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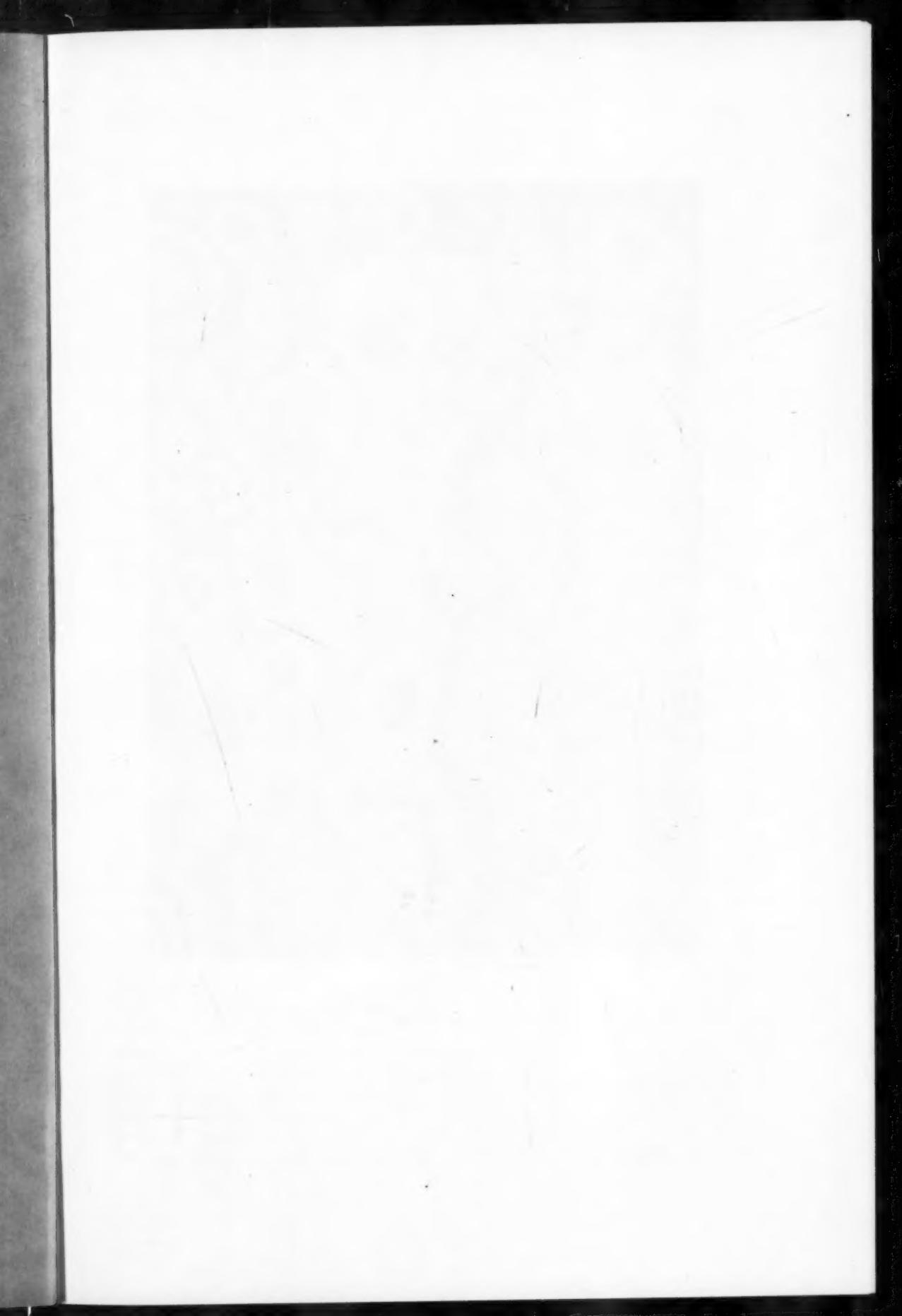
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Charles S. Myers

# THE PSYCHOLOGICAL REVIEW

CHARLES SAMUEL MYERS

1873-1946

On Tuesday, October 8, 1946, Dr. Charles Samuel Myers was a guest of honor at a luncheon held at the Mansion House, London, in celebration of the 25th anniversary year of the National Institute of Industrial Psychology of Great Britain. In replying to a toast to the National Institute of Industrial Psychology, as its founder, Dr. Myers recalled an experience as a member of the Cambridge Anthropological Expedition to Torres Straits and Sarawak in 1898:

"I recall," he said, "my efforts on one occasion to induce an old savage named Ulai to part with his tally of love conquests, a bundle of sticks in which each such episode was scored by a notch cut in them. I wanted to bring this tally—*Kupe* was its native name—back to England as a museum piece, but old Ulai would part with it only grudgingly. As at last he handed it over, he gazed at it with a melancholy expression, saying, 'Me old man now.' So I am inclined to feel today, although in fact I am far from 'parting' with the Institute."

Dr. Myers returned to his home at Winsford, near Minehead, on Wednesday morning, took to bed, and died suddenly, early on Saturday morning, October 12, 1946. His death ended the career of England's most eminent psychologist, born on March 13, 1873, whose life span almost coincides with that of the experimental science which he so well represented and whose activities and achievement kept pace with the

strides made by the science itself. The heritage he leaves is not "a museum piece," but an active group of well-trained students and a variety of establishments to carry on the work in which he pioneered.

The character of Dr. Myers' interests and accomplishments can be summarized in two facts. As Reader of Experimental Psychology at Cambridge University, he developed and directed a laboratory which still remains the outstanding center of research in experimental psychology in Great Britain. As a founder of the National Institute of Industrial Psychology, he was responsible for the establishment of a center of research in applied psychology second to none in Great Britain and outstanding among research institutions of its kind in the world. The breadth of interest underlying these two accomplishments is the keynote to Dr. Myers' professional career. Engrossed during the first two decades of the century with the development of facilities for research in 'experimental' psychology, he foresaw the need and opportunities for applied research in a variety of fields. Working actively during the past 25 years in furthering applied psychology, Dr. Myers consistently remained aware of the need for basic research and of the essential interdependence of research in so-called *pure* and *applied* science.

Two chapters in Dr. Myers' *An Introduction to Experimental Psychology*,

published in 1911, are devoted to a discussion of mental tests. Writing on industrial research in 1925 (*Industrial Psychology in Great Britain*), shortly after he had effected a transition from the quiet laboratory to the hubbub of industrial activity, he stated, "not only . . . is each industrial investigation a research, but it is continually looking to the pure sciences of psychology and physiology for guidance, and it is, at the same time, continually revealing wide gaps in their knowledge, and suggesting important problems for laboratory research." In an article published posthumously in 1947, 'An analysis of intelligence: a critical notice,' the work of Meili is reviewed from the viewpoint of a psychologist who had been long concerned with the philosophical as well as with the experimental aspects of mental organization. This keen appreciation of the interrelation of basic research and practical applications, together with capacity for original investigation, critical insight, skill in writing for both fellow scientists and laymen, and ability as an organizer and leader, combined to place Dr. Myers at the head of British psychologists in contributing both to the development of the science and to its useful application in everyday life.

Dr. Myers started his education at the City of London School. In 1891, at the age of 18, he entered Gonville and Caius College, University of Cambridge, receiving the A.B. degree in 1895 (A.M., 1900; Sc.D., 1909). Interest in the biological sciences at Cambridge led to enrollment for medical training at St. Bartholomew Hospital (M.B., B.Ch., 1898; M.D., University of Cambridge, 1901). However, training in medicine was never considered as leading to a career as a physician, but merely as a way of obtaining the additional training in physiology and allied sciences which Dr. Myers considered desirable preparation for research in psychology.

Actually Dr. Myers' first direct contact with research in psychology came through his participation in an anthropological field study and not through contact with the medical sciences. In 1898 he joined W. H. Rivers, and a group which also included William McDougall, as a member of the Cambridge Anthropological Expedition to Torres Straits and Sarawak. 'A study of Papuan hearing,' 'The visual acuity of the natives of Sarawak,' published in 1902, and 'Hearing, smell and taste' and 'Reaction times' (*Rep. Camb. Anth. Exp. to Torres Straits*, Vol. II), published in 1903, are reports on the early application by Dr. Myers of psychological methods in studying sensory discriminations and reaction times of primitive populations. Later publications, including *The Ethnological Study of Music* (1905), 'A study of rhythm in primitive music' (1905), 'Music' (in *The Veddas*, 1911), 'Music' (1912), while concerned primarily with music, in which Dr. Myers had more than a passing interest, reflect also his initiation into psychology through the medium of anthropology.

The first distinctive stage in Dr. Myers' career as a psychologist, after his work with the Cambridge Anthropological Expedition, includes approximately 20 years during which, following a short period of service as house physician at St. Bartholomew, he was engaged in academic work as an assistant to W. H. Rivers at Cambridge (1902), as Professor of Psychology, Kings College, London (1906-1909). He returned to the University of Cambridge to become Demonstrator and, finally, Lecturer and Reader in Experimental Psychology and Director of the Cambridge Psychological Laboratory. This is a period during which Dr. Myers devoted his energies to building up the Psychological Laboratory and to establishing the pattern for teaching and

research in experimental psychology in England described, in part, in his *Textbook of Experimental Psychology* (1909) and *An Introduction to Experimental Psychology* (1911). His publications during these years and until approximately the beginning of World War I were concerned chiefly with studies in sensory discrimination, although references in his texts to mental tests, and an occasional article such as 'Pitfalls of mental tests' (1911), give evidence of a growing interest in applied psychology which was uppermost in the later years of his life.

The period between the beginning of World War I and its end was essentially a period of transition in Dr. Myers' career. Entering the services as a civilian registrar in a base hospital in France, Dr. Myers was later commissioned Lt. Colonel (R.A.M.C.), serving finally as Consultant Psychologist to the British Armies in France. 'Contributions to the study of shell shock' (1915, 1916, 1919) tell of his work during this war and describe important strides, made in association with W. H. Rivers, towards the application of psychological techniques in the diagnosis and treatment of war induced neuroses and towards the analysis of mental abnormality. Contacts with problems of personnel selection during the war, as well as his practical experience in the field of medical psychology, appear finally to have focussed the interest of Dr. Myers in the practical applications of psychology and set the stage for the move, to use his words, from "the fairly peaceful academic life at Cambridge in *pure psychology*" to "a wider, less tranquil life in *applied psychology* in London."

The final phase in the career of Dr. Myers begins with the organization, in association with H. J. Welch, of the National Institute of Industrial Psychology. Dr. Myers served as the Director

of the Institute (1921-1930) and later as its Principal (1930-1938). The object in establishing the Institute was to create an agency for psychological research and practice to be supported by grants from firms and private individuals and, in part, by charges made to industrial firms and other agencies in whose behalf investigations were undertaken. Years of teaching and research in psychology had prepared him for the formulation of the research objectives and for the administration of the research program. Through visits to industrial and commercial establishments he quickly acquired the background and skill in dealing with practical situations required for the stimulation of interest among industrial leaders in the everyday use of psychology for the selection, training, and maintenance of personnel. In such contacts he succeeded in both enlisting and retaining the interest and coöperation of businessmen, while continually stressing his conviction that "research work and the discovery of the practical techniques to which it leads and the giving of instruction in these techniques" were more important than casual services rendered by the Institute to industry and to the public in general. As a result, the National Institute of Industrial Psychology succeeded not only in helping individual firms, but also in developing new techniques and methods of investigation which represent a contribution to the theory and to the content of psychology.

These contributions have been described in the books and articles which deal with many aspects of industrial psychology, including *Mind and Work* (1921), 'Industrial fatigue' (1925), *Industrial Psychology in Great Britain* (1925), *Business Rationalization* (1932), *Ten Years of Industrial Psychology*—with H. J. Welch (1932), 'Industrial psychology in the modern world' (1937), *Psychology as Applied*

*to Engineering* (1942). At the same time, evidence of continued interest in the broader aspects of general and experimental psychology is to be found both in technical publications, such as 'A theory of sensory adaptation' (1921), 'Some present tendencies in psychology' (1925), *Psychological Conceptions in Other Sciences* (1929), 'The relation of acts and contents of consciousness' (1933), 'Is the doctrine of instincts dead' (1942), as well as in more popular publications, such as *In the Realm of Mind* (1937), which treated a variety of psychological topics in a manner that helped to promote the insight of the man-on-the-street into the problems, methods, and accomplishments of psychology.

In 1938 Dr. Myers turned over to younger men the management of the National Institute of Industrial Psychology while continuing, as Honorary Scientific Adviser, to give the staff the benefit of his experience, and to serve also as editor of *Occupational Psychology*, the journal of the National Institute of Industrial Psychology. With the onset of World War II, Dr. Myers was again drawn into active work in applied psychology as a member of an Advisory Committee set up by the War Office to advise on psychological matters, particularly on the selection and allocation of personnel. In addition, he was in close contact with psychological work in the Navy since selection and placement in this service were handled by a department staffed largely by personnel from the National Institute of Industrial Psychology loaned to the Admiralty for that purpose. According to C. B. Frisby, present Director of the National Institute of Industrial Psychology, these staff members profited not only from the training which many had received under Dr. Myers, but from the advice and help which he gave to them throughout the war.

Training of personnel to conduct work in the field of applied psychology, conducted at the National Institute of Industrial Psychology and through teaching at the London School of Economics following his separation from Cambridge, represents not the least among the important contributions made to British Psychology by Dr. Myers. These contributions did not remain unrecognized during his lifetime. He was one of the first of the psychologists to be elected as a Fellow of the Royal Society. He was the first president of the British Psychological Society; President of the Psychological Section, British Association for the Advancement of Science (1922, 1931); President of the Psychiatric Section, Royal Society of Psychology (1923); President, International Congress of Psychology (1923); Honorary Member, Société de Psychologie de France; Editor, *British Journal of Psychology* (1911-1924). He was granted honorary degrees by the University of Manchester (Sc.D.), the University of Calcutta (LL.D.), and by the University of Pennsylvania (Sc.D.), the last on the occasion of the University's bicentennial celebration.

The greatness of Dr. Myers as a psychologist was matched by the completeness of his family life and personal associations. He is survived by his wife, two sons, and three daughters. The eldest son is a regular Army officer who attained the rank of Brigadier during World War II and was one of the liaison officers in Yugoslavia during 1943 and 1944. The second son holds an executive position in one of the large insurance companies. He leaves also a large number of professional associates and friends who recall him as a modest, diffident person who combined with great qualities of mind and wide range of cultural interests a quiet sense of humor, well balanced tolerance, and

gentle ways. Among those who will fondly recall Dr. Myers are the many refugees from Europe seeking an asylum in England whom he assisted during the war. As one of his associates writes, he "always seemed accessible, always helpful and sympathetic, evoking from his staff and associates not only deep re-

spect, but real affection." Those who have had the privilege of working with and knowing Dr. Myers through the years are aware of a deep sense of personal loss, as well as of a major loss to British psychology, in his death.

MORRIS S. VITELES

*University of Pennsylvania*

## SCIENTIFIC MODELS AND HUMAN MORALS<sup>1</sup>

BY GORDON W. ALLPORT

*Harvard University*

Within the span of remarkably few years, the quantity and quality of investigations in the fields of personality and social psychology have established not only their scientific dignity but likewise their popularity and promise within the psychological profession. The official formation of this large Division within the American Psychological Association is a formal recognition of these facts.

At the same time the significance of this occasion extends beyond the boundaries of the profession. In forming this Division we are, wittingly or unwittingly, stating our readiness to assume a certain responsibility. We are announcing, in effect, that as a group of scientists we believe we have a contribution to make in interpreting and in remedying some of the serious social dislocations of today. For if we did not believe in the potentialities of our science would we thus formally establish it?

The test of our fitness to exist and to prosper, I submit, will be our ability to contribute substantially in the near future to the diagnosis and treatment of the outstanding malady of our time. The malady I refer to is not war, for modern warfare is but a symptom of an underlying morbid condition; it is not the threatening fission of one world into two, ominous as this threat may be; nor is it our apparent inability to control for our safety and profit the transformation of matter into atomic energy, though this crisis too is now upon us.

<sup>1</sup> Address of the Divisional President before the first annual meeting of the Division of Personality and Social Psychology of the American Psychological Association, September 4, 1946.

I speak rather of the *underlying* ailment, of the fact that man's moral sense is not able to assimilate his technology.

While technological warfare, technological unemployment, and the atomic age—all by-products of physical science—have overtaken us, mental and moral science have made no corresponding gains in allaying the rivalries and anxieties induced by technology, in devising methods of social control, nor in enhancing human coöperation and solidarity. It is, I venture to point out, precisely our own young science, whose formal establishment we are now celebrating, that has failed to keep pace with the needs of the times.

In taking stock of the situation I observe how many of us seem so stupefied by admiration of physical science that we believe psychology in order to succeed need only imitate the models, postulates, methods and language of physical science. If someone points out the present inutility of mechanical models in predicting any but the most peripheral forms of human behavior, we are inclined to reply: Wait a thousand years if necessary and you will see that man is a robot, and that all his mental functions can be synthesized in kind as successfully as we now synthesize table salt, quinine, or a giant calculator. While we righteously scorn what one of us has called "the subjective, anthropomorphic hocus pocus of mentalism" (6), we would consider a colleague emotional and mystical should he dare speak of "the objective mechanomorphic hocus pocus of physicalism."

Let our progress be gradual, we say. By sticking to peripheral, visible operations we may some day be able to ap-

proach complex problems of motivation, and then come within hailing distance of the distresses of mankind. We hope that these distresses will keep a thousand years until we are ready to cope with them, and that in the meantime a free science will be permitted to linger along and take its time. But even if such improbable conditions were fulfilled, I question whether we should endorse this counsel of patience or the premises upon which it rests.

The machine model in psychology had its origin not in clinical or social experience, but rather in adulation of the technological success of the physical sciences. Since psychologists, like everyone else, are enmeshed in the prevailing ethos, they too, unless especially on guard, are likely to allow their subservience to technology to outrun their moral sense.

Besides the mechanical model, there are two other currently popular paradigms in psychology that are, in my opinion, only slightly less inept in guiding significant research or theory concerning the foundations of social morality. I refer to the phylogenetic model and to the infant mind. Although both these patterns during the past two generations have brought new insights and correctives into our work, they have not proved adequate to the needs of clinical, personnel, and social psychology.

#### THE CURRENT APPEAL TO PSYCHOLOGY

Public officials, confronted by post-war dilemmas, are urgently seeking the aid of psychologists. Many of us who have been approached are embarrassed by the scarcity of scientific findings, and even of serviceable concepts and well-formulated problems, that psychology has to offer of the type that is being sought. What is asked for is instant help in discovering the sources and conditions of man's moral sense in order that this sense may be enlarged and

brought into focus. What is asked for is aid from a science of human relationships whose assistance Franklin D. Roosevelt likewise invoked in his last speech before his death.<sup>2</sup> Yet we may comb the entire file of the *Psychological Abstracts* and find very little that has any bearing upon the improvement of human relationships on an international scale.

Why have we so relatively little to offer? Is it that we are young and need to follow the machine model for a thousand years? Or have we gotten off to a thoroughly bad start through our adoption of root-metaphors that lead away from, rather than toward, the problem at hand? Three generations ago psychology was commonly classified as a "moral science." Though we may not favor the aura of this term, how can we expect anything other than a science of moral conduct to discover conditions that will bring the needed counterpoise to technology run wild?

When any one of us undertakes a piece of research he inevitably adopts, according to his preference, one or another of the fundamental models available to psychologists. My thesis is that now if ever we need to test our preferred model for its capacity to yield discoveries that have some sure relevance to moral nature and to social skills.

#### EXPECTANCY AND INTENTION

If I interpret the matter correctly, American psychology naturally adopted mechanical models because our culture has always been action-oriented and technological. By and large our psychology is a motorized psychology, and is only now widening its concept of ac-

<sup>2</sup> "Today we are faced with the preëminent fact that, if civilization is to survive, we must cultivate the science of human relationships—the ability of all peoples, of all kinds, to live together and work together, in the same world, at peace."

tion to include the ego-involved participation of the human organism in matters affecting its own destiny (2). The earlier extreme position, represented by E. B. Holt and J. B. Watson, held personality to be essentially a battery of trigger-release mechanisms. This view paid no attention to the sustained directions of striving characteristic of moral behavior, to what in this paper I shall call "intentions."

This trigger-model, still preferred by a few, gave way gradually to a more purposive behaviorism. The concept of "sign-Gestalt expectancy" was introduced by Tolman, and mercifully shortened by Hilgard and Marquis to "expectancy" (9). It is an interesting fact that these authors seem to regard the principle of expectancy as the most purposive of all the essentially mechanical theories derived from the multitudinous experiments on the conditioned reflex (9, p. 101). In other words, some version of the principle of expectancy is as far as many psychologists have come in their conception of the nature of personal and social conduct.

The principle holds that in the presence of certain signs the organism expects a certain goal to appear if it follows the customary behavior route. If the goal is reached, the expectation is confirmed; if not, the organism may vary its behavior (9, p. 88). The principle, while allowing for the importance of attitude, is essentially stimulus-bound. We behave according to the cues we have learned, according to our expectancies.

In order not to complicate my argument I shall leave out of consideration the law of effect, which, it would be easy to show, likewise ascribes behavior wholly to past experience, to learned cues, and to mechanical reinforcements (4). Both principles, so far as I can see, accord nothing to the unrewarded,

unrealized, yet persistive, intentions of man's moral nature.

The trouble with these currently fashionable concepts, drawn from the phylogenetic model, is that while they seem to apply aptly enough to animal behavior whence they were derived, they have only a limited or else a remote analogical bearing on the activities of human beings. We may know a person's expectancies and even his past rewards, and yet be singularly unable to predict or control his future behavior, unless at the same time we know also his basic intentions which are by no means a stencilled copy of his previous expectancies and rewards (3).

To take an example, the sign-Gestalten today are such that we may now reasonably expect future trouble with Russia. Does this fact tell in any degree what we can, should, or will do about it? This precise area of conflict is a novel one (as indeed all important situations are). The best predictive basis we have lies in our own national and personal *intentions* regarding Russia. It is our purposes, not our expectancies, that are now the issue.

As if aware of the scantiness of the expectancy principle, Tolman advises us to embrace also a "need-cathexis psychology" (19). But the situation here turns out to be parallel. Need-cathexis psychology—of course I oversimplify—holds essentially that a handful of physiological drives get attached to this, that, or the other object. A man who, in Tolman's pleasing vernacular, is "raised right" meshes his drive into a socially acceptable gear. A man "raised wrong" does not. But what is so striking about human motivation is that so often a desire or aspiration is meshed into no gear. It simply reaches forward hungrily into the future like the tip of a scarlet-runner bean groping for a goal that it does not know about.

The embarrassment of the need-ca-

thesis type of psychology is reflected in the apologetic language it uses when referring to this expansive aspect of human motivation. Accustomed to work with animals or with infants, need-cathexis psychology labels adult human intentions "secondary drives," "derived drives," or "drive conversions." With such depreciating concepts both the mechanical and the phylogenetic psychologists apparently seek to dispose of those morally relevant desires and aspirations that are in fact so different from the drive-impelled excursions of the cozy robot or cozy rodent.<sup>3</sup>

My objection to the animal paradigm

<sup>3</sup> It is instructive to read the perorations of two recent presidential addresses by psychologists, one preferring the machine model, the other the rat model. Though good-humored and witty, both authors candidly acknowledge their own escapist motives. To paraphrase Carlson's quip concerning Cannon's theory of emotions: the authors seem to entertain their models because the models entertain them.

"I believe that robotic thinking helps precision of psychological thought, and will continue to help it until psychophysiology is so far advanced that an image is nothing other than a neural event, and object constancy is obviously just something that happens in the brain. That time is still a long way off, and in the interval I choose to sit cozily with my robot, squeezing his hand and feeling a thrill—a scientist's thrill—when he squeezes mine back again" (2, p. 192).

"And, as a final peroration, let it be noted that rats live in cages; they do not go on binges the night before one has planned an experiment; they do not kill each other off in war; they do not invent engines of destruction, and if they did, they would not be so dumb about controlling such engines; they do not go in for either class conflicts or race conflicts; they avoid politics, economics and papers on psychology. They are marvelous, pure and delightful. And, as soon as I possibly can, I am going to climb back again out on that good old phyletic limb and sit there, this time right side up and unashamed, wiggling my whiskers at all the dumb, yet at the same time far too complicated, specimens of *homo sapiens*, whom I shall see strutting and fighting and messing things up, down there on the ground below me" (19, p. 166).

for personality and for social psychology is not so much that animals lack culture—a fact which Mr. Tolman in his sparkling paper first frankly admits and then amiably represses. My objection is rather that the motivational structure of man and of lower animals seems to be in only a slight degree similar. In this respect as with his evolutionary brain development, "Man," to quote Julian Huxley's conclusion, "stands alone" (12). Animals are demonstrably creatures of stimulus-expectancy and need-cathexis. Man, in all that is distinctive of his species, is a creature of his intentions. We may well doubt that the basic equation for intentional morality, or that for intentional learning, can be written from a study of organisms that lack propositional symbols. To this point I shall return.

While I am disapproving of current models I shall state my final grievance, this time against the rigid ontogenetic stencils that derive from Freudianism. Odd as it may appear, Freud resembles the mechanical and phylogenetic psychologists in wanting his doctrine of motivation anchored to neuro-anatomy. I assume that this is his desire because of his refusal to see anything at all in the coöperative, socialized, affiliative, undertakings of mankind excepting goal-inhibited sexuality. To the sex drive he adds principally the impulses of aggression, destruction, and death. It seems obvious that Freudianism, even though eagerly adopted by many who have found the mechanical and animal models inadequate, offers an equally meagre basis for a serviceable study of man's moral conduct.

The trouble lies chiefly in the excessive emphasis upon infantile experience. We are asked to believe that an individual's character-structure is, in all essentials, determined by the time his last diaper is changed. Even Suttie, who

postulates as the foundation of morality an original and embracing instinct of tenderness, affection, and social symbiosis, believes its fate is sealed according to the manner in which the mother handles this affiliative impulse before and after weaning (17). If the chances for peace in the world depend to such a degree upon infant fixations ought we not disband this Division and register as wet nurses to the mewling citizens of tomorrow?

The concept of intention, which I am here opposing to reactivity, expectancy, and infantile fixation, is not immediately congenial to American psychology. Yet its adoption in some form or another, I argue, is necessary. With some malice aforethought I have selected the term *intention*—spiced, as it is, by an aggravating flavor of mentalism—to signify those aspects of thought and of motivation that play a leading, but now neglected, part in the complex, affiliative, moral conduct of men. I believe it is precisely the "private" worlds of desire, aspiration, and conscience that must be studied if we are to succeed in the task of social engineering.

In using the term intention, however, I am not arguing surreptitiously for phenomenology, though in order to improve our grasp on the subtleties of man's intentions we would do well to emulate the refinement of its descriptive method.<sup>4</sup> Nor am I arguing for a revival of Brentano, though we have neglected unduly the central proposition of

<sup>4</sup> An excellent example is Bertocci's analysis of man's sense of moral obligation (5). He shows that when we study the *ought-consciousness* phenomenologically we discover how entirely different it is from the *must-consciousness*. This discovery leads to a justifiable suspicion that, whatever conscience may be, it does not derive merely from fear of punishment or from social coercion. Too hastily and heedlessly have psychologists accepted Freud's identification of the Super-ego with threat of parental punishment.

Act Psychology: that at every moment man's mind is directed by some intention, be it loving, hating, comparing, understanding, desiring, rejecting, planning, or some similar mental act.

Let us define intention simply as *what the individual is trying to do*. Naïve as this definition may sound it is in reality the product of decades of sophisticated wrestling with the problems of human motivation. In this concept influences as diversified as Brentano, Darwin, Freud, Cannon, and Wertheimer are brought into focus. In essence it no longer draws the sharp distinction, advanced by both Kant and Schopenhauer, between will (or drive) on the one hand, and intellect on the other. The machine, rat, and infant models we have been following (though I am sure they'd be surprised and grieved to know it) preserve this irreconcilable Kantian dichotomy. They side somewhat more, however, with Schopenhauer in regarding the functions of the intellect as wholly instrumental and secondary. Without forgetting for a moment what we have learned about rationalizing and about the untrustworthiness of introspective reports on motives, we may safely declare that the opposing of motive and thought-process has gone much too far. Usually the individual is trying to do something in which his wants and his plans easily coöperate. Instead of being at opposite poles his emotion and his reason canalize into a single endeavor. The direction of his endeavor I designate as the intention, and offer this concept as an improvement upon the one-sided irrationalistic doctrines of drive, need, instinct, and cathectis.

In deference to the discoveries of psychoanalysis we readily admit that an individual does not always know precisely what his own intentions are. *Consciously* he may misinterpret the line of his own endeavor. A neurotic

frequently does so. In such cases insight is either lacking or partially lacking. But as a rule, the "posture or lay of consciousness" reflects accurately enough that inextricable fusion of driving and planning which we find in the dynamics of mature human conduct.<sup>5</sup>

It is the mark of an intention that it is directed toward the future. Yet it is typical of the models we have followed that they lead to preoccupation with adjustments in the past. While people are living their lives forward, psychologists are busy tracing them backward. The model we need for our investigations of human relationships will escape from our present excessive dependence on geneticism in all its forms (3).

A geneticist, for example one who places great weight on the expectancy-principle, is inclined to define personality as a peculiar set of reaction-tendencies. An intentionist, on the other hand, sees personality as a peculiar set of subjective values. There is a difference. The one learns at best only about moral accomplishment; the other gains additional light on moral potential.

<sup>5</sup> McDougall specifically objected to the concept of intention on the grounds that conscious intention merely obscures the instinctive motive at work (15, pp. 121f). He had in mind the indubitable fact that men's verbal reports of their intentions may be rationalizations. But in my use of the term I do not confine intention to reportable purpose. Sometimes the essential direction of an intention is understood well enough by the subject, sometimes not. If the term, as I propose, is taken to mean both the understood and non-understood direction of an act I maintain that it can serve as a proper designation for "ultimate motives" and not merely for proximate or rationalized motives.

To my mind it is unnecessary to have recourse to a doctrine of underlying needs or instincts. McDougall, for example, allowed far too little for the ever-changing panorama of man's intentions which, as they evolve from an original genetic equipment, undergo complete change of form and functional significance (1).

It may be argued that the models I am presuming to criticize do deal both with "goal reactions" and with "anticipatory goal reactions." Dr. Hull, for example, offers "anticipatory goal reaction" as a "physical mechanism" which he says he regards as equivalent to the concept of "guiding ideas," or what I am calling *intention* (11). The difficulty with "anticipatory goal reaction" as with "expectancy" is that men often have values without having any specific goal in mind. They may have a consistent direction of striving, but their goals are either transient or else undefinable. All of a rat's, but only a small bit of human, behavior can be characterized in terms of concrete goals whose attainment will de-tension specific drives. For the most part the course of man's behavior runs according to certain schemata, or in prolonged channels. Only now and then are these channels marked by lights or buoys that represent specific goals.

A simple example may be borrowed from Lecky's analysis of childhood thumbsucking. The following statement distinguishes neatly between expectancy and what I am here calling intention; that is, between behavior regulated by habit and behavior ordered to non-specific schemata.

"Certainly the child who sucks his thumb gives the act plenty of exercise and gets enough satisfaction from it to fix it indelibly. Therefore if the habit theory is true, we should be able to predict absolutely that the child will continue to suck his thumb for the rest of his life. But what really happens? Every year millions of children who have industriously sucked their thumbs since birth, and who have successfully resisted every effort to force them to change their behavior, quit the practice spontaneously when they are five or six years old. The reason is that they are beginning at this age to think of themselves as big boys or girls, and they recognize that thumb-sucking is inconsistent

with the effort to maintain this new idea" (13, p. 122f).

An intention often takes the form of a self-image as in the case of Lecky's reformed thumbsucker. Having adopted a conception of what we want to be we are constrained to make good in the role we have assumed. The specific goals we set for ourselves are almost always subsidiary to our long-range intentions. A good parent, a good neighbor, a good citizen, is not good because his specific goals are acceptable, but because his successive goals are ordered to a dependable and socially desirable set of values. We now know that juvenile delinquency and adult criminality were sadly misconceived so long as they were regarded as a matter of bad habit-formations. For years reformatories have trained habits, but have achieved few reformations. Only a radical shift of outlook and intention remakes a criminal, alcoholic or neurotic character.

The models we have been following lack the long-range orientation which is the essence of morality. Infant and rodent have immediate goals and indulge in anticipatory goal reactions, but have no directive schemata. By contrast, a child in puberty develops a desire to become a successful and respected man of affairs, and acquires this generalized objective long before he knows what concrete goals he has to work for. Thus customarily, image and intention seem to antedate and to define goal-reactions. The essence of moral behavior is of this sort. It presupposes long-range purposes whose directions precede their specifications.

When President Roosevelt enunciated the Four Freedoms he was speaking of certain common intentions of the human race. An important feature of his historic formulation lies in his assumption that *all* men, in *all* cultures, intend (that is, long for) freedom from

want, freedom from fear, freedom of speech and of worship. Note how this assumption contrasts with the prevailing creed of modern social science. Cultural relativity, really a doctrine of stimulus-expectancy, has laid such a heavy hand upon us that we have overlooked the possibility of universal intentions. Yet unless Roosevelt's bold assumption is found justified, we can scarcely hope to find a psychological basis for effective world organization.

In all probability Roosevelt's formulation is psychologically not the best that can be made; nor dare we underestimate the incompatibility of nationalistic intentions and rivalries. What I am saying is that the psychologists' perspective should be equally bold. It is up to us to find out whether there are in fact common purposes that might provide ground for international solidarity. To do so, social psychologists in all lands might well join in a search, through modern instruments of polling, clinical interviewing, child study, and life-histories, for existent moral bases on which international coöperation can be built.

It is conceivable—I think probable—that such research would discover the ruthless pursuit of personal and national power to be a result of the frustration of basically affiliative intentions. In clinical practice we know how often the clamorous manifestations of egotism gain the upper hand when men are denied a proper continuation of the originally friendly and symbiotic relationship with family, friends, and neighbors. It seems probable that every child in every nation, the world over, at a time when he is most plastic, wants security, affection, and an affiliative and comprehending relation to the surrounding world. It is conceivable that the same basic intentions exist in most adults, although thwarting and perversion of this relationship have engendered a vast

amount of hatred, emotional instability, and warlike impulse.

Basic research would discover why the taboo on tenderness, on nurturant desires, has grown so excessive that the development of coöperative and affiliative behavior outside one's own family is, at least in our culture, generally disapproved. It would seek to discover under what conditions the impulse to love and to be loved is turned to the impulse to hate and to invite hatred. If it is the child's nature to trust everyone, why is it the nature of national or ethnic groups to distrust nearly everyone? The models we have been following tend to deflect our attention from problems of human affection and the conditions for its development. When a bit of human friendliness is discovered—and it can be discovered only accidentally with models now current—it is likely to be labeled "goal inhibited sexuality," and thus tagged, forgotten. Up to now the sexual activity of rat and man has received incomparably more attention from psychologists than has the coöperative activity of men and nations.

Besides the study of affection and hatred, the possibilities for peace require research into many other strictly human capacities—among them the use of humor, the function of creeds, the processes of communication. For moral development depends on many factors other than root-desires and intentions. But every aspect of moral conduct that one can name depends intricately upon the employment of symbols.

#### SIGNS AND SYMBOLS

Perhaps the clearest symptom of the present conceptual confusion in our field is the extent to which we confound symbols with signs, or—if one prefers Morris's terminology—symbols with signals.

We know that all animals, as well as men, respond to signals. The principle

of expectancy says so, and in this respect is right. A signal is something that exists in the physical world; it is an identifiable stimulus. But even the most behavioristically inclined theorists cannot, and do not, claim that animals can handle propositional symbols—those self-produced signs of signs which are man's prized and troublesome possession. An animal, says Thorndike, can "think things," but it cannot "think about things" (18, p. 119). And Yerkes asserts that symbolic processes in chimpanzees are rare and difficult to observe. One may, he says, fairly continue to question their existence, though it may be that signal responses can be regarded in some way as "antecedents of human symbolic processes" (20, p. 189). Surveying relevant investigations and opinions, Cassirer concludes:

"In all literature of the subject there does not seem to be a single conclusive proof of the fact that any animal ever made the decisive step from subjective to objective, from affective to propositional, language" (7, p. 30).

Cassirer argues, reasonably enough, that the symbolic system creates a wholly new dimension of reality for man. Instead of dealing directly with things themselves or with their visible signals, man deals with their ideational surrogates.<sup>6</sup>

"He has so enveloped himself in linguistic forms, in artistic images, in mythi-

<sup>6</sup> Even in human beings we occasionally encounter a sharp break between symbols and signs. Some of Goldstein's aphasic patients, for example, seem capable of responding to signs but not to symbols, as in the case of the man who could understand the word-signs "Drink it," when a glass full of water was presented to him, but was unable to go through the symbolic motions of drinking it if the glass was empty (8, p. 44).

Without symbols we could not make-believe, dissimulate, or lie; we could not form plans for our future; nor hold those schemata in mind that make possible consistency in moral conduct.

cal symbols or religious rites, that he cannot see or know anything except by the interposition of this artifical medium" (7, p. 25).

Even so behavioristic a writer as Morris admits that the theory of sign-response as developed by himself carries over with difficulty to the human sphere. These are his words:

"... non-human beings seldom produce the signs which influence their behavior, while human individuals in their language and post-language symbols characteristically do this and to a surprising degree. Here is a basic difference between men and animals, and until behavioral theory develops a semiotic adequate to this difference it will remain what it is today: a careful study of animals and a pious hope for a science of the human person" (16, p. 198).

In this passage Morris seems to be saying with fine candor that there is a world of difference between signal and symbol; and that even his own careful system of semiotic fails adequately to bridge the gap. Though I have not actually counted the illustrations in his recent book I have the impression that a majority of them refer to animal responses to signals, and that relatively few deal with human responses to symbols. In any case it is clear that Morris, like many psychologists, is enamored of the phylogenetic model.

I venture to cite another brilliant and candid passage from his book. He writes of the fact that a sign may be *iconic*, that is to say, it may itself resemble the properties of its denotatum. Thus a motion picture is highly iconic; an onomatopoeic word less so; a wholly arbitrary sign not at all iconic. He then goes on to make this highly significant remark:

"One of the dangers of the use of models in science, for instance, arises out of the temptation to ascribe to the subject matter

of a theory properties of the model illustrating the theory which are not involved in the theory itself" (16, p. 23).

From this warning would it not follow that an adequate theory of symbols can hardly be derived from the animal model in which *signals* alone predominate? How can we expect to understand human symbolism in terms of the phylogenetic type when, as Morris himself asserts, we are tempted to over-extrapolate the properties of our type-model and force them to serve in place of the independent theory that we need to develop?

#### THE MODEL WE NEED

To sum up: the designs we have been using in our studies of motivation, of symbol, and hence of the foundations of moral behavior, are not—to borrow Morris's crisp term—sufficiently iconic with our subject-matter. Addiction to machines, rats, or infants leads us to overplay those features of human behavior that are peripheral, signal-oriented, or genetic. Correspondingly it causes us to underplay those features that are central, future-oriented, and symbolic.

What sort of a model then do we need? This question opens systematic vistas that lie beyond the scope of this paper. Yet, lest my numerous criticisms indicate a despair that I do not actually feel, I shall mention a few recent signs and portents that signify a newer—and, to my mind—more wholesome outlook.

Most noteworthy is the fact that the war led many psychologists to deal directly with the integrated behavior of GI Joe, of the factory worker, of the civilian. We then learned that the interests of morale, psychotherapy, personnel placement, psychological warfare, could not be pursued successfully by clinging to our threadbare models. Our

inadequate root-metaphors went into the ash can for the duration. It is because of this conceptual discard, with its resultant wartime success in the promotion of social engineering, that I have presumed at this time to bring into the open a conflict that many, perhaps most of us, have secretly felt. Must we now resume the tattered stencils that we so recently abandoned with such good effect?

There are various indicators of improvement in theoretical outlook. I have in mind the new and vital conception of the ego that has come into psychotherapy in recent years (3); the discovery and application of psychological principles involved in bringing the worker into a participant relation with his job (2); the discovery and application of procedures leading to successful administration (14). We discern an accelerated movement toward the development of such theories as can have their acid test here and now, not one thousand years hence. These theories neither strain the credulity, nor stretch an inappropriate model some distance beyond its logical breaking point.

We happily find more emphasis than before on the structuring activities of the person, on the importance of centrally initiated motive patterns, on cognitive dynamisms—including ideology, schemata of meaning, frames of reference. We find the contemporaneity of motives stressed, as well as the important functions of self-esteem and ego-involvement. Though symbols are still confused with signals, we are beginning, through content-analysis and interviewing, to study symbols both in their own right, and as the basic ingredients that they are in all complex conduct, including all morally relevant thought and behavior. We have learned, through improved polls and other methods of inquiry, to ascertain the direction of so-

cial purpose as it resides in individual minds. From such knowledge it should be possible to fashion a domestic and international social policy that will be sufficiently realistic to succeed.

All these and many more signs indicate the growing dependence of modern theories upon a model that is none the less scientific for being humane. As this design for personality and social psychology gradually becomes better tempered to our subject-matter we shall cease borrowing false notes—whether squeaks, squeals, or squalls. We shall read the score of human personality more accurately, and for the benefit of the world audience that waits to listen.

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# THE PHENOMENOLOGICAL APPROACH TO SOCIAL PSYCHOLOGY

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Social psychology to-day is both one of the most challenging and one of the least disciplined fields of psychological research. The rapidly growing interest in the psychological study of the individual in society reflects a healthy moral awakening on the part of psychologists to their social responsibilities. Yet, as we review some of the great issues towards which psychology should contribute understanding, *e.g.*, the causes of national, racial and class tension, the possibility of individual freedom in a tightly organized society, we cannot but be appalled by the discrepancy between what we can now assert as established fact or principle and the insights which society rightly expects us to provide. The simple fact is that the sudden expansion of the field of social psychology finds us with conceptual and methodological tools which are inadequate to the new task. Social psychology must either rest content with present concepts and methods, which have proved useful in restricted contexts, and run the risk of producing obvious or trivial results, or make a fresh start, reviewing its basic assumptions, redesigning old tools and inventing new ones, and developing a science equal to the task which confronts us.

The writer believes that in this connection the social psychologist can profit from a glance at recent developments in the psychology of perception. In some respects social psychology to-day resembles the psychology of perception of thirty-five years ago. At that time the systematic application of the phenomenological method liberated the psychology of perception from its traditional

bonds and launched it on a new and productive course. Whether or not an analogous method can be successfully applied to social problems remains to be seen. The present thesis is that it must be tried. The systematic application of a social phenomenology holds forth the promise of a new definition of the field of social psychology, of a more adequate social psychological methodology, and ultimately of an integration of the laws of social psychology with those of perception.<sup>1</sup>

## I. PHENOMENOLOGY IN THE PSYCHOLOGY OF PERCEPTION

By the phenomenological method,<sup>2</sup> as applied to psychology, is meant the sys-

<sup>1</sup> The direct application of perceptual principles to problems of social psychology has already proved fruitful, *e.g.*, in the work of Sherif (8). The importance of a phenomenological approach to social problems has been stressed by various authors, *e.g.*, Köhler (6, 7), Koffka (5), Heider (3).

<sup>2</sup> Psychological phenomenology must be clearly distinguished from philosophical phenomenology (2). While the former derived historically from the latter, and can continue to profit from the developments in philosophical phenomenology, it has remained and must continue as an approach rather than as a theory. Phenomenology in psychology can never be a substitute for psychophysics and psychophysiology. Its function is rather to set the initial problem, to define the psychological datum which must then be envisaged in the setting of its physical and physiological correlates. Whereas philosophical phenomenology began with an attempt at an unbiased description of psychological data and proceeded therefrom toward an account of ultimate reality in terms of its essences, the psychological phenomenologists have endeavored merely to liberate their science from some of its theoretical biases, and to focus attention on

tematic attempt to observe and describe in all its essential characteristics the world of phenomena as it is presented to us. It involves the adoption of what might be called an attitude of disciplined naïveté. It requires the deliberate suspension of all implicit and explicit assumptions, e.g., as to eliciting stimulus or underlying mechanism, which might bias our observation. The phenomenological question is simply, "What is there?", without regard to Why, Whence or Wherefore. In a sense every psychologist is at times a phenomenologist, and no psychologist ever achieves the ideal. Nevertheless it is an ideal which can be approached; and it was the application in systematic and rigorous form of the phenomenological method to the study of perception that marked a turning-point, about thirty-five years ago, in the history of that branch of psychology. Although the importance of a psychological phenomenology had been recognized long before by Hering (1878), had been expounded by Husserl (1900), and was reflected in the early researches from the laboratories of Stumpf, G. E. Müller and Külpe, it can scarcely be disputed that the publication of Katz' *Erscheinungsweisen der Farben* (4) and of Wertheimer's study of movement (10) demonstrated for the first time how a careful phenomenology could lead to a thoroughgoing reformulation of the whole problem of perception.

The psychology of perception of

problems which might otherwise be neglected. Thus, for example, the phenomenal properties of subjectivity and objectivity, of reality character, of psychological distance, which tend to escape the notice of the seeker for simple physical and physiological correlates of experience, are seen by the phenomenologist in their proper perspective. And there is no reason to doubt that, in due course, we shall have a psychophysics and psychophysiology of these properties analogous to our present psychophysics and psychophysiology of color.

thirty-five years ago had notable achievements to its credit. With methods adapted from physics and sensory physiology it had demonstrated that mental phenomena can be studied under controlled laboratory conditions. It had successfully challenged the age-old assumption that psychological data can not be measured. It had succeeded, within limits, in refining out of the older association psychology, with its confusion of logical with psychological analysis, a set of constructs which could serve as a framework for a validly psychological description. And it had formulated and tested a set of principles which promised, if systematically applied, to lead eventually to a complete and scientific account of perceptual experience.

The achievements of Titchener, of Wundt or of Helmholtz should not be disparaged. Yet it was in a sense the very excellence of their work which hampered the forward movement of the psychology of perception. They had provided a neatly hewn set of building blocks with which the edifice of perceptual experience was to be reconstructed. Elementary sensations were the building blocks. These elementary sensations had been studied in the laboratory, their properties described, their physical and physiological correlates measured. One cannot condemn the faith that every perceptual experience, however complex, could finally be reduced to terms of elementary sensations combined in a particular way. It was natural that Helmholtz, for instance, should insist that the apparent discrepancy between physical change and phenomenal change in what has subsequently been called color constancy involved no real change of sensation, but was due rather to an elaborate system of unconscious inferences. The hypothesis of a simple point-to-point correlation between stimulus and sensation

had proved satisfactory in the laboratory study of elementary sensations. When a more complex phenomenon was presented it was easier to maintain the original hypothesis, and to add to it the postulate of certain secondary psychological processes, than it was to take a fresh look at the phenomena and redesign the hypothesis accordingly.

The phenomenologically minded students of perception had three interrelated biases to combat: (1) the belief that something small is more fundamental than something large—*the atomistic-reductive bias*; (2) the belief that a differentiable variable in the physical world or a differentiable receptor in the organism implies the existence of a correspondingly differentiated unit of experience—*the stimulus-receptor bias*; and (3) the belief that that which is genetically early is more fundamental than that which is genetically late—*the genetic bias*.

The atomistic-reductive bias was assimilated directly from 19th century physical science. In an atmosphere dominated by the search for physical elements it was natural that psychologists should seek for the elements of mind and attempt to reduce complex mental phenomena to terms of those elements. It is equally intelligible, if less pardonable, that even after physics had in large part given up its elementarism psychologists should continue to believe that only by reductive analysis can psychological phenomena be explained.

The stimulus-receptor bias was in part a by-product of the laboratory method, in part merely an unchallenged consequence of the Aristotelian doctrine of the senses. Throughout the history of psychology the senses, at first five and later many more, have been accepted as the mediators between the mind and the external world. Succeeding studies of the anatomy and physiology of the sense organs have per-

mitted closer and closer correlations between physical stimulus variable and elementary sensory reaction. Granted the search for elements, it is understandable that that experience was considered elementary which seemed to be the direct result of the activation of a simple receptor mechanism. By the same logic, any experience which evinced no such simple correlation was accorded a secondary status. Thus there was imposed on the perceptual world a theoretical structure which did not necessarily correspond to the structure of direct experience.

The genetic bias is, if possible, even more tenacious than the other two. The 19th century emphasis on evolution is merely a vigorous expression of a much older belief that if we are to understand we must seek origins. The search for origins frequently involves the implicit assumption that an earlier form of a phenomenon is somehow or other a truer form. In perception the assumption was that the perceptions of everyday life are tainted with past experience, that the only true immediacy is found in a perception when it is occurring for the first time. Hence the tendency to regard generalizations based on meaningful material as suspect; hence the unwillingness of psychologists in their study of perception to deal with phenomena of everyday life.

The genius of the phenomenological psychologists lay simply in their ability to suspend these biases while they took a fresh look at the world of perceptual phenomena. And the results were impressive. One need only recall the early contributions of Bühler, Gelb, Jaensch, Katz, Koffka, Köhler and Wertheimer—to mention only the leading names—to realize that new and challenging problems were emerging in fields which had been rendered inaccessible by the older point of view. Color constancy is a good example. The basic

facts had been observed by Helmholtz, and both he and Hering had advanced alternative explanations. It was Katz, however, who recognized that an adequate treatment of the problem required a revision of the descriptive methods of color psychology. The simple categories of hue, brightness and saturation afford no basis for an intelligible characterization of surface quality, film quality, bulkiness, shininess, transparency or luminosity. Some colors, for example, are perceived as properties of objects (the whiteness of a sheet of paper), others as properties of media through which objects are seen (the redness of a stained glass through which the paper is seen), and still others as properties of empty space (the blueness or greyness of the sky); and the behavior of such colors depends on their mode of appearance. The whiteness which inheres phenomenally in a sheet of paper is more resistant to change than is the brightness which appears as a property of the illumination. If conditions of observation permit, a decrease in the intensity of the light reflected by the paper will tend to be seen as a darkening of the illumination rather than as a darkening of the color of the paper. The 'true' color of the paper will tend to maintain itself as long as possible. Thus visual perception responds selectively to changes in physical stimulation. It is as though the organism in its perceptual activity were attempting to resist the disrupting influence of change by referring changes to the functionally less important parts of the field. The same generalization is supported by observations in other fields of perception, notably the perception of the size and shape of objects, of position in space, of induced movement, and even of such object properties as temperature and apparent loudness.

The theory of the constancies need

not be discussed here.<sup>3</sup> There are, however, a few general facts worth noting: (1) The problem can receive no constructive formulation as long as psychological thinking is dominated by the reductive-atomistic, the stimulus-receptor and the genetic biases. From the traditional point of view it represents merely a set of perceptual anomalies, analogous to the optical illusions, which must be 'explained away.' (2) Once the problem has been accepted as legitimate, phenomenological observation reveals as relevant a series of perceptual properties which have been disregarded in traditional psychological analysis. (3) These 'new' perceptual properties—which, in fact, are all familiar phenomena of everyday observation—prove in the main to be properties of field organization which disappear under traditional conditions of laboratory observation. To study them in a scientific way the experimenter is consequently required to revise his experimental methods. (4) Any analysis of the field determinants of an individual phenomenon leads to a new appreciation of the interdependence of different parts of the perceptual field, and ultimately to the formulation of principles of field organization which, in turn, (5) make new demands on psychophysical and psychophysiological theory.

The constancies represent only one group of problems which, formulated first as a result of phenomenological analysis, have contributed to the development of a field theory of perception the implications of which extend far beyond phenomenology. The whole configurational emphasis in psychology presents a similar history—first an attempt at an unbiased description of phenomena, then systematic experimentation designed to reveal the essential

<sup>3</sup> For a fuller discussion of the problem see Koffka (5) and Woodworth (11).

determinants of these phenomena, and finally revision of existing theory in the light of the new principles discovered. A phenomenological emphasis in psychology does not restrict the psychologist to the description of phenomena. It requires him, however, to look first at the world of things-as-they-are in its entirety before deciding which aspects of this world are to be considered important for theory, and at every stage in his investigation to keep checking back to phenomena to make sure that they have not been distorted by the very process of investigation. In essence the phenomenologist asks the question 'What?' before he asks the questions 'Why?', 'Whence?' or 'Wherefore?', and his answers to the latter questions are guided by his answer to the first.

## II. IMPLICIT ASSUMPTIONS IN SOCIAL PSYCHOLOGICAL THINKING

How should one proceed with a phenomenological analysis of the social world? The first fact to be noted is that there is no social world different from or superimposed upon the world of perception. We are dealing with a single set of phenomena, and we shall presumably discover a single set of laws. We are striving merely to complete a phenomenological analysis which has hitherto recoiled from some of the more difficult phenomena. In the second place, the phenomena which we call 'social' are intrinsically more difficult to describe, both because of the poverty of our language (which is in itself of great phenomenological significance) and because the phenomenologist here meets in its most acute form the old difficulty of being at the same time both subject and observer. In the third place, these very descriptive difficulties make it even more necessary than in standard perceptual analysis that the descriptions of one observer be checked against those of other observers.

If we are to follow the pattern of perceptual phenomenology, our descriptive analysis must be preceded by an attempt to uncover and temporarily to suspend the implicit assumptions, or biases, which govern our social psychological thinking. The following list of biases is offered with no claim to completeness or originality, and without any suggestion that they necessarily represent unsound approaches. No science can proceed without systematic assumptions. What is important, however, is that our assumptions be held up to scrutiny before they are accepted as a basis for scientific thinking. The following biases are presented in simple and dogmatic form as representative ways in which the subject-matter and methods of social psychology may be predetermined by a set of implicit assumptions.

(1) *The organism centered bias.* The essential determinants of social behavior are defined as conditions of the organism or as forces emerging from the organism. The language of instincts, drives, needs, interests, attitudes and other inherited or acquired dispositions of the organism, accords primary significance to directive and regulatory factors of a biological or quasi-biological order, and relegates to a position of secondary importance extra-organismic direction and regulation. In Lewin's terms this is to recognize vectors from within as more fundamental than vectors from without; in Murray's terms it is to make need more important than press. In a very general sense it is to found our social psychology on a psychology of motivation. Such a tendency is recognizable, for instance, in the explanation of social behavior as the expression of a gregarious instinct or an instinct of imitation, or in the acceptance of a postulated trait of suggestibility as the basis for responsiveness to propaganda.

(2) *The genetic bias.* The explanation of a present state, e.g., a conflict, a prejudice, a loyalty, is sought in the forces which operated at an earlier period in time rather than in the forces at present operative. Although the enthusiastic search for social origins which characterized the social anthropology of the late 19th century has been generally replaced by a more functional approach, and the animal psychologists have long since lost their conviction that human social behavior can be seen in its 'raw' form in the animal world, the ontogenetic bias is still strong. The question 'Why?', applied to present behavior, suggests a 'because' which points back to early childhood. The implication is that the true pattern of behavior is revealed in the child, and that present behavior is composed of the original plus modifications or accretions. Without disparaging genetic studies we must insist that continuity does not necessarily imply identity in any essential respect. When I say that I still possess my great-grandfather's axe, which incidentally has had two new heads and five new handles since my great-grandfather's time, the historic reference has little bearing on the present properties or functions of the axe. The significance of present behavior in its present context, e.g., the anarchist's resistance to political authority, may be obscured by a too ready identification of it with a similar pattern, e.g., an Oedipus conflict, in the context of childhood.

(3) *The sociological bias.* This is somewhat analogous to the stimulus-receptor bias in the psychology of perception. In its most common form it involves the acceptance of the structures and processes of society as defined by the sociologist as the true coordinates for the specification of social behavior and experience. From this point of view, for instance, the church or the po-

litical party in which the individual possesses membership is regarded as an institution of society, possessing the manifold properties and functions which a many-sided sociological investigation reveals, rather than as the church or political party as it is apprehended and reacted to by the individual. The process of social adjustment, of socialization or of attitude formation thus becomes defined in terms of a set of norms which have reality for the scientific observer, but not necessarily for the individual concerned.

(4) *The logical bias.* Although this is most commonly found in the psychology of motivation it may, by virtue of that fact, affect social psychological observation and thinking. The most obvious example is found in the characterization of the directedness of motivated behavior in terms of a goal which is a logically implied outcome of the behavior but which is not in any sense psychologically contained within the behavior. Thus to assert that the goal of an act is to attain a final state of equilibrium or of maximum happiness, or to preserve the species, or even to escape from a conflict, is to impute a directedness to behavior which may not be present as such to the individual. Granted that the problem of unconscious motivation, and consequently of unconscious goals, is not to be lightly brushed aside, it would seem plausible that the attack on the problem of directedness should begin with the study of goals which are phenomenally present. To argue from correlational studies, for instance, that anti-negro prejudice is based on the desire to eliminate the negro as an economic competitor is to translate a set of relevant conditions into a possibly fictitious goal structure. This may or may not be a real goal, or it may be one of a number of equivalent goals subordinate to a more general goal. We cannot solve the problem of

prejudice until we discover what goals are actually present and how they are related to one another.

(5) *The reductive-atomistic bias* may affect social psychological thinking as it does every other kind of psychological thinking. Although the sensation units of the traditional theory of perception are no longer relevant, it is still easier to break social behavior down into small parts, into response mechanisms, into discrete attitudes or into cultural patterns, and to treat these parts as independent existents with their own inherent properties and functions, than it is to preserve the more cumbersome language of functional interdependence. We are tempted to assume, for instance, that because our language is studded with familiar names for social objects—races, nations, classes, and so forth—and because meaningful reactions can be elicited by these names, that therefore there must exist an attitude corresponding to each name, each attitude constituting a separately measurable entity. Such an assumption can lead to a *Stückhaftigkeit* fully as dangerous as that contained in associationism. Although this danger has been clearly recognized (1, 9), the prevalence of the bias cannot be overlooked.

(6) *The relativistic bias* might be considered as the logical opposite of the reductive-atomistic bias. This has gained support in recent years from anthropological studies. The demonstration that particular customs or particular personality traits become fully intelligible only in the light of a particular cultural pattern leads readily to the conclusion that there are no 'absolutes' in human nature. Insofar as this represents a recognition of the facts of functional interdependence it can be accepted as a healthy reaction against atomism. In its extreme form, however, it may lead to an equally untenable assumption, namely, that no thing, proc-

ess or situation possesses any inherent properties. The fact that what we condemn as 'stealing' may be applauded in another culture suggests a relativistic interpretation of crime. What is relative to the culture, however, is merely the particular act which is condemned as criminal, not the fact of criminal behavior as such. The term 'crime' represents a pattern, found in all societies, in which certain acts are condemned as bad. It has a meaning which is independent of any particular cultural setting.

Both atomism and relativism can be avoided if we take another look at phenomena. The fact is that the phenomenal world contains things which possess properties phenomenally inherent in the things, but it also has characteristics which are accidental and extraneous to thinghood. It is neither an array of arbitrarily definable entities nor an infinitely complex net-work of relationships. An adequate phenomenology should give us, at least as a basis for further inquiry, an indication as to which phenomena represent tough and resistant entities or systems to which causal significance can be attributed and which phenomena represent fleeting functions of field conditions.

### III. SOME SAMPLE PROBLEMS FOR A SOCIAL PHENOMENOLOGY

The listing and scrutinizing of biases is only a first step, preparatory to the phenomenologist's attempt, with these biases held temporarily in abeyance, to give a systematic account of all the social phenomena of his world,—describing, classifying, analyzing, abstracting, generalizing, and so forth, and eventually emerging with a conceptual scheme which will serve as a framework for empirical investigation. It is obvious that this is not the way in which a social phenomenology will be worked out. The phenomenologist must begin with

specific problem areas, and gradually extend the scope of his inquiry. The following paragraphs will briefly annotate three such problem areas, namely, (1) the self as phenomenal datum, (2) the other person as phenomenal datum, and (3) society as phenomenal datum. In each case the lack of an adequate phenomenology has seriously crippled the advance of social psychology.

(1) *The self as phenomenal datum.* The term 'subjective' is, of course, to be encountered everywhere in the literature of psychology and philosophy, ranging in its use from an epithet connoting 'unscientific' or 'unreliable' to a technical term designating a school of philosophy. In the classic arguments of the British empiricists the perceptual properties of objects were variously considered as inherent in the object, *i.e.*, really there, or dependent on the perceiver, *i.e.*, subjective. Thus the distinctions among primary qualities (*e.g.*, extension, duration), secondary qualities (*e.g.*, color, sound) and subsequently tertiary qualities (*e.g.*, beauty, ugliness), were based on their presumed degree of dependence on the perceiving mind, or subject. Such a dependence was, however, an inferred dependence, not a perceived dependence, and the resultant confusion in epistemology has never been completely dispelled. In recent psychology the problem has been more frequently stated in terms of degree of dependence on the organism—with the same resultant confusion. It becomes speedily clear that any act or experience of an organism is dependent on the organism, and that any attempt to differentiate exactly between the contributions of the organism and those of the environment is doomed. Thus, in the traditional sense, all psychological data become 'subjective.'

Let us, however, suspend for a moment our concern about dependency on the organism (or the perceiver) and

look at the phenomena themselves. What, for instance, are the phenomenal differences between a color and a pain? The color is usually 'out there' in space; it is usually a property of an object; I am convinced that it is still there even when I close my eyes; I cannot apprehend it as a property of me. On the other hand, my headache is mine; the pain of the pin-prick is felt in me, not in the pin; and try as I may I can only seldom and with difficulty 'push out' the pain and regard it as something independent of me. From a psychophysiological point of view, both color and pain are in pretty much the same sense dependent on the organism. From the phenomenological point of view, there is a clear and inescapable difference. The color is objective; the pain is subjective. Thus objectivity and subjectivity are phenomenal facts; they represent a dimension of variation in the phenomenal world just as valid as those of brightness and loudness. Every perceptual experience can be rated in this way, although no perceptual quality considered in abstraction can be assigned a fixed place on the scale. Thus the perceptual result of patterned cutaneous stimulation may change rapidly from objective roughness or smoothness to subjective pressure.

Subjectivity and objectivity are properties of an organized perceptual field in which the points of reference are selves (subjects) and objects, and the degree of articulation in this dimension may vary greatly. It is probably true that no object-organization can exist without a self as a point of reference, and that no feeling of selfhood can be maintained without at least some degree of organization in the area of the not-self. There are, nevertheless, conditions, as for example in extreme drowsiness, in which the distinctions are blurred, and it is probable that the world of the child is less differentiated and stabilized in

this respect than is the world of the adult. It may be, in fact, that a line of psychological development could be traced, along which self and object could be represented as becoming progressively differentiated out of a more primitive state of consciousness in which neither exists in its characteristic adult form. Whether or not this be true, it is clear that self and object have many characteristics in common. Each constitutes a segregated entity in the same perceptual field, oriented and extended in space, persisting in time, maintaining its identity in spite of changes in the field, and so forth. In traditional language, each possesses primary and secondary qualities, many of which are similar. It is in the category of the tertiary qualities that the differences are greatest; but even here most of the differences are differences of degree. A phenomenal object may be attractive or repellent; objects may interact with each other in a way which is perceived (not merely inferred) as causal; the behavior of another person may be perceived (not merely inferred) as directed toward a goal. Such properties are validly objective. In the ordinary perceptual situation, however, they pale before the corresponding properties of the self. Perceived causality in its most impressive form is found in the experience of self-determination; perceived purpose in the experience of striving. In hate, fear and curiosity the properties of the object are far from neutral; but the self is by far the liveliest component of the field.

It is this very predominance of the dynamic properties of the self which leads us so readily to the assumption that all dynamic and directive characteristics of experience are in essence subjective, that the world of objects is a neutral screen upon which we project our own hopes and fears. In the psychology of perception this is a serious

error; in social psychology it is fatal. Let us consider, for example, what is commonly called a race prejudice. In colloquial language, I dislike the members of another race. I consider them intellectually and morally inferior, I shun their society, and I actively resent and resist any suggestion that they be considered as my equals. Let us assume that a careful investigation reveals no significant differences between the disliked race and my own. The prejudice must, ergo, be a matter of my own 'subjective' attitude, and any effort to eliminate the prejudice must accordingly be focussed on 'my' attitude. Such an effort will usually take a form varying from moral exhortation to forthright denunciation. The implicit assumption is that 'my attitude' is a possession of me, and that pressure on me will change it. The actual result of such pressure may be to create an attitude which is genuinely 'mine' and which will consequently be protected with all the resources of my self.

Even a superficial phenomenological analysis will throw light on the problem. How much of the prejudice is validly subjective, in the phenomenological sense, and how much objective? When we see an apple as red and appetizing, we apprehend the redness and the appetizingness as in the apple, not in ourselves. These are objective properties. Furthermore we may apprehend redness as related to appetizingness in a way in which brownness would not be; and whether or not that relation is historically connected with previous experiences with apples is irrelevant so far as the present objectivity of the percept is concerned. If we wish to change the redness or the appetizingness of the apple we must change the context within which it is seen. Similarly we may apprehend another person as brown-faced, dishonest and stupid, and all these properties may be as validly objective as are

the properties of the apple. Our dislike will be felt as caused by, not as causing, the perceived properties of the person; and our dislike will continue until the person is apprehended in the kind of context which will present him with a different set of phenomenal properties.

What we call an attitude is thus, phenomenologically considered, not a state of the self but a state of the 'field,' of which the self is a part; and it may well be that the most important components of the attitude are in this sense objective rather than subjective. It is clear that the relative prominence of subjectivity and objectivity may vary considerably from attitude to attitude and from person to person. We are all familiar with the 'pet prejudice' which becomes almost a possession of the self. It would seem probable, however, that most of our attitudes have developed, and still exist, as objective, cognitive structures, or even as ill-defined cognitive properties which have not yet become structures. If this be true, the first task of the psychology of attitudes must be cognitive analysis. The psychologist must first seek to determine, without regard to conventional attitudinal categories, exactly what is there for the person, what structures with what properties and related in what ways. What we need is thus a kind of descriptive analysis of the objective field which is unbiased by hypotheses about our deeper motivation.

(2) *The other person as phenomenal datum.* The other person is, first of all, a segregated object in the perceptual field. He can be seen, heard, touched, recognized as familiar, differentiated from other objects. To this extent he presents no new problem. Other objects can be attractive, mobile, threatening. At first glance it might appear as though persons could be classified merely as a sub-category of objects-in-general, differentiated from other ob-

jects by their possession of a particular combination of dynamic properties, all of which are present in lesser degree in other objects. And a strong case might be made for such a thesis. For the child the distinction between person and thing is less clear than it is for the adult. For the savage the inanimate world may be highly charged with dynamic properties. Rather than assume that the child and the savage have projected their own feelings into the inanimate world, it would make more developmental sense to say that as we have become sophisticated we have depersonalized an originally animate world.

For a preliminary analysis, however, the *origin* of the 'personal' properties of the other person is an irrelevant question. The fact remains that as a person he may have a unique status among objects in the psychological field. The other person is, more or less, 'like me'; he can be apprehended as an 'other self.' Regardless of the specific ways in which he may differ from me, in what are commonly called personality traits, he may appear to me irresistably as someone who can initiate action, who can be kindly or cruel, who can rightly be praised or blamed for what he does. The stubbornness with which the problem of the freedom of the will maintains its challenge is testimony not only to the experienced fact of self-determination but also to the experienced fact of other-self-determination. Inanimate objects can be apprehended as causes and can be believed to incorporate immanent or transcendent purposes, but the other person is there for us as an object which can generate its own purposes. In the context of the other person, purpose may thus be a phenomenologically objective property. Recognition of this fact need not force us into a teleological position.

Within the general category of 'other persons' we can distinguish great differ-

ences in the degree to which other persons can be apprehended as 'other selves.' The patient in a catatonic stupor, for instance, is apprehended indubitably as a person, but we cannot easily apprehend him as in the same class with ourselves. The same applies to the new-born child. To a lesser but significant degree we have difficulty in accepting as an 'other self' a New Guinea native or a member of another national group; and we may even experience this difficulty with a member of our own sociologically defined community whose habits, beliefs and tastes differ widely from our own. This general fact has been widely recognized, for instance, in the studies of social distance, in such distinctions as that between in-group and out-group, in the psychological treatments of sympathy and antipathy and of the sentiment of self-regard; but it deserves a more elaborate phenomenological analysis.

The way in which we apprehend the other person is basic to the dynamics of inter-personal relations, to the group-structure of the world of people as we see it, and, very practically, to the way in which social tensions develop and are resolved. Consider, for instance, the mechanism of guilt projection. However we may characterize it in detail, it involves a reorganization of the psychological field of such a nature that the self remains relatively intact and the responsibility for our reprehensible conduct is assumed by some entity in the field other than the self. But all objects in the field are not equally receptive to the projected guilt. The ideal recipient is that object which possesses the characteristics of another self. The factors which determine 'other selfhood' are thus the factors which determine the direction of the projection. The same holds true for the experience of collective responsibility. When my colleague behaves badly I am ashamed. I am

ashamed when my congressman casts a cowardly vote, when a scientist of another nation yields to fascist pressure. When the New Guinea native strangles his wife, however, I am not ashamed; for there is no psychologically real way in which I can include the New Guinea native with me in a common category of 'we.' The experience of collective responsibility can extend itself only within whatever limits are indicated by the term 'we'; and 'we' can refer only to myself and other persons who are apprehended as other selves.

Such examples could be easily multiplied. Our immediate concern, however, is merely to point out: (1) that within the category of psychologically apprehended persons there is a range of variation which extends from persons who are relatively neutral or indifferent to us to persons who are highly charged with selfhood; (2) that these phenomenal differences betoken crucially important determinants of our behavior with reference to other persons and, ultimately, of the way in which the social world becomes organized for us; and (3) that a thoroughgoing phenomenology of the other person is consequently basic to social psychology.

(3) *Society as phenomenal datum.* Society is composed of people; but the social world as we apprehend it is only in small measure composed of persons. In face-to-face social situations, it is true, we deal directly with other persons; the individual to whom we address a letter may appear to us as a person; and so may the leading statesman of another country as we reflect upon the tension between his country and ours. Such persons may be objects of admiration or contempt, hate or reverence, or indifference, and whether or not they are perceptually present, they may regulate our behavior in a personal way. But for most of our social behavior other persons as segregated

phenomenal objects constitute little more than anchorage points, the most vividly presented components of vague and fluctuating social structures. When I tremble before public opinion I may have a particularly spiteful spinster lady in mind; but the public opinion as such has for me an undeniable, if somewhat intangible, reality which extends beyond the spinster lady. When I resent the interference of the church in political affairs I may have fleeting memories of good and bad priests whom I have known, which may correspondingly allay or reinforce my anxiety; but beyond the priests is a social structure which is 'there' for me and which has properties of its own. The sociologist in his analysis of society may recognize such structures as family, occupational group, political association, religious organization, nation or racial group, or he may specify social constraints in terms of custom, tradition or law. It would be ridiculous to expect that every sociologically definable institution or function must have its counterpart in the social world of the individual, or even that the formal pattern of psychological analysis should coincide with that of sociological analysis. What is important for social psychology, however, is the fact that the social world of the individual is structured. Thus our basic phenomenological question, "What is 'there' for the individual?", becomes in this context "What is the social structure of the world he is living in?"

The practical importance of this kind of social phenomenology is so evident that it should need no demonstration. The individual for whom China is represented by the corner laundryman and Russia by the bewhiskered terrorist of the cartoons is living in a world quite different from that of the student of Chinese literature and Russian military strategy. Any attitudes which they evince towards China and Russia can-

not be adequately understood until we know what cognitive structures are represented by these words. The methodological difficulties, however, are formidable. Is it possible to give a disciplined yet unbiased description of the psychological structure of the social world? In what terms should such an analysis be made? Where should one begin? Disturbing as these questions may sound, they are in principle no different from the questions which one must ask in connection with any phenomenology; and one must answer them in the same way. Predisposing biases cannot be excluded from the psychology of perception, but by becoming aware of them we can partially control them. When we describe a perceptual object or event we do in fact lift it partially from its context and thereby run the risk of suppressing or distorting the very phenomena which we wish to observe; but we can guard against the danger by systematically shifting from one level of description to another. All phenomenology inevitably involves analysis, but we can always distinguish, in principle at least, between an analytic procedure which is arbitrary and destructive and one which is dictated by the phenomena themselves. Social phenomenology is in this respect no different from perceptual phenomenology. When we open our eyes and look upon the world immediately about us, we find objects, events and relationships thrusting themselves upon us. These are the subject matter of perception psychology. When we look beyond our immediate surroundings, we find larger contexts of objects, events and relationships which less vividly but equally truly thrust themselves upon us. Their organization may be unclear and unstable, and they may almost defy description. Yet these are facts, and they constitute an essential part of the subject-matter of social psychology.

Social phenomenology presents two special difficulties, however, which deserve mention. In the first place, although the analysis of perceptual phenomena requires no hypothesis about the physical counterparts of those phenomena, we do in fact believe that the phenomenal rests on the physical, and we use our knowledge of the physical to create or to modify the phenomena which we wish to observe. Thus the constructs of physics provide us with a base-line on which to plot our phenomenal variables. For a social phenomenology there is no such base-line. We may believe, as we all in fact do, that nations, political parties and churches, have real existence, but their precise definition is still a matter of dispute, and we cannot manipulate them as we do lightwaves and electric currents. It can be argued, of course, that the constructs of sociology are strictly analogous to those of physics, since each science represents a transcendence of the world of phenomena; but sociology is still in the descriptive stage, and part of that description rests on the very social phenomenology which we are now trying to develop.

The second difficulty is more serious. The content of the perceptual world can be defined roughly as that which is here and now, and it is usually anchored to a physical reality which affects us directly in a physical way. The social world may include the here and now, but it extends far beyond both in time and in space. In functional terms the social world is a product of memory and imagination rather than of perception. This does not mean that it may not be stably structured. When we say that we have firm convictions about the proper settlement of the Arab-Jewish question we are indicating the existence of a stable structure in our social world, although none of the components of the situation may be present in a perceptual

sense. Purely fictitious objects, events and relationships can be just as truly determinative of our behavior as are those which are anchored in physical reality. Our difficulty lies, however, in the fact that social phenomena are for the most part present to us only partially or in symbolic form. The social world is too large to be encompassed in a single act of apprehension. As it becomes articulated during the course of our development its various structures become salted away in the form of linguistic symbols. The fact of linguistic stereotyping of social structures is, of course, not to be deplored. It is the basis upon which we maintain ourselves in a world which extends beyond the perceptual. But by the same token we run the risk in our social phenomenology of mistaking a set of linguistic stereotypes for a description of what is genuinely there, of accepting, for instance, an expanded definition of the formal concept of 'country' as a substitute for the psychological structure 'our country' which is there for us in our social world. Granted that linguistic categories may serve to select, strengthen and even at times to create the structures of our social world, and that for this reason linguistic analysis may contribute richly to social phenomenology, the fact nevertheless remains that the word can indicate but never completely represent the phenomenon. We may apprehend phenomena for which we have no adequate linguistic symbols; and we may use language with formal correctness but without any clearly articulated field of reference. Our almost paradoxical task is, while of necessity using language in our analysis, to penetrate through language to real psychological structures, yet all the while recognizing that some linguistic artifacts are psychologically real.

Social psychology must simply recognize these two difficulties and make the

best of the situation. We have no independently established social categories for which we can find psychological counterparts. Nor can we simply sit back and describe the social field as we would describe an array of colors, permitting the natural organization of the field to dictate our descriptive categories. Our necessary compromise is on the one hand to accept the conventional social categories simply as points of departure for an analysis of the psychological structures which roughly correspond to them, and by a process of descriptive elaboration and correction to work towards a more valid set of psychologically defined categories; on the other hand to select deliberately social situations which do not fit readily into the conventional categories of social analysis and from these to glean hunches as to principles and patterns of organization which are ordinarily overlooked. The two approaches are complementary. If followed systematically they will eventually give us an analysis of the social world which is validly psychological. A few examples may clarify the point.

We may attempt, for instance, to construct a psychological geography. An orthodox geographic analysis will give us as units continents, countries, subdivisions of countries, cities, rivers, etc., with each structure formally comparable with any other structure of its class. But what is actually there for the individual? Certainly not all the facts discussed in the school text on geography; and, even if we remember as many facts about Bolivia as we do about Sweden, this has little bearing on the relative importance of these two countries in our psychological world. What we must find out is how clearly Bolivia exists for us as a structure, what properties it possesses, to what extent it is differentiated from or grouped with other countries; in short, what

Bolivia means to us. Thus if we go systematically through the categories of the geographer we can gradually construct a useful, if partial, picture of the geographic world as we apprehend it. It will be something like the famous 'New Yorker's map of the United States,' but it will be closer to the psychological truth than any geographer's map will be.

Similarly we may try to construct a psychological history, linking the past with the present in terms of developments not as they 'really' occurred but as they are apprehended by us. '1066 and All That' is an essay in psychological history which can be read with profit as well as with amusement. Our present attitudes towards the UN, Russia, Great Britain, are embedded not in 'real' history, but in history as we know it, the history of the League of Nations, of World War I, of the Russian revolution, of the American Revolution, constructed in terms of personalities, events, causes and consequences as we apprehend them. In the same way the concepts of local community, negro race and owner class are represented in the social field by structures which may resemble only crudely the institutions and groups as defined by the sociologist. Nevertheless it is these, and not the hypothetically 'real' structures, which actually regulate our social behavior, which constitute the objective components of our loyalties, our prejudices and our resentments.

Such an inventory of social structures would in itself, however, be inadequate; for it would fail to indicate which of the structures are functionally important, and it would not reveal psychological structures for which there is no conventional symbol. The fact, for instance, that psychological structures can be found which correspond roughly to 'Bolivia' or 'ownership class' may be quite misleading, for they may have

little importance as regulators of behavior, and they may indeed be crystallized into structures for the first time by the question which is asked. Thus as a supplement to the inventory analysis we must have an approach which is less constrained by conventional categories. Such an approach might, for instance, be a variant of the free interview method. If we ask a child to compare his negro and white playmates we may elicit a negro stereotype, echoed from his parents, which in fact does not constitute an important structure in his field. If, instead, we ask him who his playmates are, which he prefers, and why, we may discover groupings which are far more important for the child than those defined by the color line, *e.g.*, children who are bullies, children who know how to play baseball. Similarly, if we ask a member of the surface gang in a nickel mine what he understands to be the essential differences between capital and labor, we may momentarily crystallize for him a class structure which ordinarily plays no role as a determinant of his behavior. His significant memberships may be with the native-born as opposed to the immigrant groups, with the Evangelical Methodists as opposed to all other religious sects, or with the old-timers as opposed to the newcomers. Or it may be that a class-structured society, which we accept as a sociological fact, is utterly unintelligible to him.

The social field of any individual is inevitably structured. We can easily see how the salience of structures which are known to us may vary from person to person, and how structures which at a given time lack salience are nevertheless deeply embedded and capable of becoming powerfully determinative. It is more difficult for us to realize, however, that structures which have no meaning for us may be exceedingly powerful for other people, and that

structures which are obvious and unchallengeable to us may be unintelligible to others. The best examples of this are to be found in the anthropological literature. The strange tabus, the peculiar religious rites, the weird beliefs of the savages, which are at first incomprehensible to us, are indicators of a psychological structure which is radically different from ours. We cannot ask the savage to phenomenologize for us, or even to explain the structures which we point out to him, since he regards them as self-explanatory. Similarly, we cannot be expected in our first attempt to give a complete account of the structures in our own field, since our description may be governed by those very structures. Nevertheless a modest achievement in this direction is possible. By deliberately attempting to step outside the context of our own culture, by attempting to adopt the point of view of another culture or of another period in our own culture and in this way seeing our own field in a new perspective, we may be able to discern as determinative psychological structures certain relationships which have hitherto gone unheeded because of their very commonplaceness. We frequently speak of the values inherent in a culture. 'The Christian emphasis on the ultimate worth of the individual,' 'the twentieth century acceptance of the Horatio Alger myth,' 'the white man's burden,' 'the age of the common man'—these are convenient clichés; but their very prevalence betokens in at least some of their users a way of apprehending the social world which must be specified if we are to understand the individuals in question.

#### IV. CONCLUSIONS

- (1) The phenomenological method, in social psychology as in the psychology of perception, can never be more than an approach to a scientific inquiry.

It is the attempt to view phenomena in their entirety and without prejudice, to distinguish the essential from the non-essential, to let the phenomena themselves dictate the conceptual framework and the further course of the inquiry. Social psychology today is suffering from confusion—as to subject-matter and as to method—and this confusion is enhanced by the sudden demands which are being placed on social psychology by an insistent post-war world. It is the contention of this paper that this confusion can be considerably reduced if we profit from the history of the psychology of perception, suspend for the time being our present theoretical constructs and take a fresh look at the phenomena which are there for us. The phenomenal world is, in fact, a social world, structured in terms of selves, other persons, groups, nations, as well as in terms of simple objects and events ordered in space and time. Social psychology must go far beyond the psychology of perception; but it deals initially with the same field, its phenomena must be describable in the same way, and its laws must be of the same kind.

(2) Our immediate need is clearly for improved methods of observation. In this connection the products of casual, uncontrolled observation must not be scorned. The alert psychologist is always in touch with his subject-matter, and much can be gained from the persistent attempt to note and describe the social phenomena which occur incidentally in one's own experience. It is notoriously difficult, however, to maintain a disciplined naïveté about oneself, and for this reason, if for no other, the social psychologist must improve his methods of studying the social field of the other person. Although most of the existing methods of social psychology, clinical, biographical, experimental, etc., can be adapted to this

end, it is suggested that the most promising method is that of the intensive interview. In the hands of the clinicians the interview has been developed as an effective technique for the untangling of confused motives. More recently the value of the intensive interview has been demonstrated in the sampling of public attitudes. In each case, although the interviewer has a limited objective, the interview permits a high degree of insight into the world of the person interviewed. For purposes of social psychology the two methods can be profitably combined, the thoroughness of the clinical interview with the objective emphasis of the survey. What we require is nothing less than a complete account of the individual's world in terms of its essential social structures.

This is, of course, an unattainable ideal; but even a partial achievement will mean progress. If we can discover some of the structures that are there for the individual, e.g., what his picture of Russia is or whether he apprehends religion as a moral compulsive, we can begin to find out why they are there, i.e., the factors which determine the development and the potency of a given structure. But in every case our point of reference must be, not some pre-established category which may be real for us, but the person whom we are interviewing. Only in this way can we establish the relation between one structure and another, and thus proceed towards the formulation of general laws of field structuring.

(3) The full implications of this approach for social psychological theory can become clear only as investigation proceeds. In the psychology of perception it has led us inevitably away from a reductive atomism towards a field theory. It is to be expected that it will have similar consequences for social psychology, providing us eventually with a set of field constructs in terms

of which all social processes can be envisaged, and ultimately with a set of constructs which will unify all psychology. The immediate implication, however, is that we must undertake a critical revaluation in the light of phenomenological evidence of such concepts as need, attitude, and value. This is not to suggest that a social phenomenology will invalidate the current research on these topics. Recent advances in the psychology of perception have not invalidated the correlations established by traditional psychophysics; they have given these correlations a new and broader significance. Similarly, many of the current measurements of attitude will stand as valid; but they may require reinterpretation. An attitude cannot be regarded either as a property of a person or as a possession of a person, any more than a perceived object can be so regarded. An attitude is a structure which has both objective and subjective components. If we are to characterize adequately an 'attitude towards religion,' we must specify both what is objectively there as symbolized by the term 'religion' and what is subjectively felt in relation to that objective structure. For some people the only clear objective structure may be the linguistic symbol, for some it may be an attractive or repulsive person, and for some it may be an elaborately organized authoritarian system which offers comfort or threatens persecution; and the degree of subjective participation may vary within an equally wide range. A need is by definition a predominantly subjective structure, a value a predominantly objective structure, but neither can be considered as exclusively subjective or objective. Thus the concepts of need, attitude and value may be ordered within a single continuum of subjectivity-objectivity.

Subjectivity-objectivity is, however, only one of many ways in which the so-

cial field must be characterized. Some structures are more tightly organized than others, more resistant to influences from the rest of the field; some are transitory, others enduring; some have a high degree of reality-character; some are strongly dynamic, are directive rather than regulative. Such concepts as purpose, belief and doubt, conservatism, loyalty, and anxiety may have their phenomenal counterparts as structures, as properties of structures or as relations between structures in the phenomenal world; or they may prove to be linguistic artifacts without meaningful phenomenal reference. The first test of their validity must come from phenomenology. As new descriptive categories emerge the traditional terms will either find their place or drop out of use.

(4) Can a social phenomenology lead to a social psychology which is useful? The answer to this question is a matter of faith. The goal of science as science is not prediction and control but understanding. Prediction is merely the test of understanding and control the practical reward. For social psychology, however, understanding itself is of immense practical value. If we can understand the world as it is structured for the other person—for the national of the country we fear, for the member of the minority group, for the revolutionary, for the reactionary, for the 'average American'—we shall have made one small step towards the resolution of the conflicts that beset us. This is, however, a mere knowledge of acquaintance, an answer to the question 'What?'; social psychology must also answer the question 'Why?'. Having described and defined the social structures which exist for us and for the other person, social psychology must proceed to the formulation of laws which govern the formation, the maintenance and the change of social struc-

tures as such. It has proved possible to formulate such laws for the field of perceptual phenomena. It should be possible for this larger field of social phenomena.

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# THE DISEASE CONCEPT IN PSYCHOLOGY

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As demon possession was gradually abandoned as an explanation of mania, melancholy, and other unaccountable dispositions of man, place was given to the disease concept as an explanatory principle. However, the concept of mental disease, as it has eventually developed, is not analogous to the physiological concept of disease, a fact not generally recognized.

To support this last statement, the current conceptions of physiological disease and mental disease will be compared. Each concept will be defined in three ways, namely, in terms of the phenomena which exemplify it, the basis for differentiating it from other physiological or psychological phenomena, and in terms of the factors which bring it about.

## PHYSIOLOGICAL DISEASE

Pathology texts usually do not contain a definition of physiological disease. The concept, merely exemplified by the contents of the texts, is that of a condition of absence of ease or comfort, of disease. In addition to pain or discomfort, a loss of efficiency or hampering alteration of function is usually observed. Such characteristics may not be present at the onset of the disease, but they usually appear eventually.

Disease is sometimes defined as a pathological condition (44). Pathology refers to structural changes in body tissue which are the material evidence of disease (34, p. 24), or its essential nature (19).

This condition which involves loss of efficiency, pain, discomfort, and involves tissue changes is a physiological process, an altered function (34, p. 23). This conception of disease as a process

is emphatically differentiated from the view that it is something foreign to the body (8, p. 5; 29, p. 124).

Conditions of disease, altered structure and function are often referred to as dysfunctions or abnormalities, thus contrasting them with normal function or health (11; 34, p. 21; 45, p. 40). Implicit in efforts to define abnormal are primitive conceptions which obscure and confuse the concept of disease rather than clarify it.

One supposed need for a criterion of abnormality derives from the etymology of the word abnormal, that is, departure from the rule. The Oxford Dictionary (32) quotes a medical use of the term, in 1835, in just this sense. It is implied that the abnormal is non-lawful and therefore inexplicable. That any function is non-lawful is of course an untenable view (15, p. 10).

Sometimes it is implied that a criterion of abnormality is required because, if a condition is abnormal, we then know that special principles are required for its explanation. This is doubtful. Such a belief is a vestige of the conception of the abnormal as non-lawful; lawfulness is admitted but special laws are thought necessary. Delafield seems to deny this necessity when he says that a disease involves "the expression of no new functional capacities which the normal body does not possess" (8, p. 6). In disease we have the manifestation of general principles of chemical, cellular, tissue, or organ function under special contingent circumstances.

These two supposed needs for a criterion of health are evidence of pre-medical, prescientific thinking. Gold-

stein (15, pp. 428-9) denies that all efforts to establish a criterion are such evidence, using the argument that disease is a fact which cannot be ignored. This argument is weak, for recognition of a fact does not imply a need for a precise definition of it. The urge to have a criterion probably is the desire for neat and orderly categories, a desire which has spurred a fruitless search for a criterion more precise than mere dis-ease.

Perkins defines health as a "state of relative equilibrium of body form and function which results from its successful dynamic adjustment to forces tending to disturb it. . . . It implies the admission of wide variability in structure and function and in the powers of adjustment within the body" (34, pp. 21-2). No generally applicable objective definition of this dynamic equilibrium exists. Later Perkins refers to normal individuals as 'apparently well-regulated bodies.'

In the phrase 'apparently well-regulated' Perkins gives tacit recognition to the fact that these bodies may not be ideal in their function; that apparent health may be to a degree maladjustive. In fact, health is an ideal. The definition of such an ideal norm is impossible and unattainable. Goldstein recognizes this and says, referring to the ideal, ". . . its frame of reference is not oriented in reality, but, rather, would have to justify itself in reality" (15, p. 427). Such a norm would be speculative and would "always differ according to the respective philosophy of life." In short, to specify such a norm would require a knowledge of the ultimate end of evolutionary development, an achievement no one would have the temerity to claim.

It is interesting to note that while Goldstein rejects the ideal norm, he is not content to accept a statistically defined one, a super-individual norm, as he calls it. We must, he says, judge

whether a disease exists only in terms of our knowledge of the individual nature. Disease endangers the 'performance potentialities' essential to the individual organization (15, p. 433). A similar view has been expressed more recently by King (25) when he urges that normal should define that function of the organism which is in accord with its design or pattern, whether or not it is statistically common. Goldstein and King are in reality objecting to the view that every deviation from that which is common or usual is necessarily indicative of disease. With this there can be no argument, yet who is to define objectively the individual's 'performance potentialities'? Kattsoff (24) has previously called attention to this weakness in Goldstein's position. There is little doubt but that optimum function is an individual matter, but to define such a function objectively is no less difficult than to define the ideal. Only experimentation will reveal how altered factors improve function or produce longevity. In this way the ideal will be approached in actuality, but not defined.

Most frequently when the question, "Is it abnormal?" is asked, reference is made to a function which is possibly premonitory of some other condition, which, if it occurs, will be recognized unquestionably by the patient or others as a disease. Pain, dis-ease, lowered efficiency, or death will be the criterion. The practical problem is one of prediction of a complex situation from the occurrence of a part-process or symptom.

In this situation, statistical norms are derived for one feature of behavior as a means of appraising the total function. Deviation of a specific function from a central tendency for that trait is evidence that a complex of factors, a disease, may exist. As Perkins states, anatomical and physiological studies of functional unities can reveal such information. Norms of blood pressure and

body temperature are results of such investigations. They result from case studies of disease, occasionally post mortem. Since not every deviation nor every degree of deviation is symptomatic of disease, that is, perfectly correlated with it, the establishment of critical points requires consideration of other sources of variance (28). The critical point is that at which the probability of disease or no disease is even. However valuable these norms of specific function may be, they are not criteria but only objective definitions of criteria which already exist and not criteria themselves.

Distinguishing disease from health and the classification of disease as abnormal adds nothing to our conception of it other than that it is unusual. However, not every unusual manifestation of function is a disease. Great superiority in strength, muscular coöordination, or motor speed is unusual, but not therefore disease. Dis-ease and lowered efficiency are the only criteria we have.

Perkins (34, p. 23) eliminates from consideration as disease those temporary manifestations which apparently right themselves. Disease is not overcome save through intervention, either by chance, or by deliberate control known as therapy.

In the place just cited, Perkins also distinguishes between disease and simple ill-health, but does not elaborate upon his reasons for so doing. However, if ill-health is a more or less constant state of the individual, it would seem to come within the scope of the disease concept. The term 'disease' is commonly confined to those syndromes which have a rather specific etiology. There is, however, no reason for so delimiting the term, and nowhere is there explicit statement that it should be done.

A fuller definition of the disease concept calls for knowledge of etiology. It

is generally agreed that disease is the organism's reaction to disturbance of its equilibrium (8, 15, 16). It is not merely the environmental condition but also the nature of the body structure which determines whether equilibrium will be disturbed and disease ensue.

Etiology is always complex and only occasionally appears to be simple. Of the causal complex, the predisposing and precipitating factors are too generally recognized to need further comment.

Perkins (24, p. 32) proposes six categories of etiological factors, namely inherited, nutritive defect, exogenous chemical agents, physical forces and energies, vital activity of invading organisms, and the psychobiologic. The last causes the most confusion for these factors involve the so-called psychosomatic relationship. The principal difficulty is obviated if a monistic position is taken and if, therefore, it is recognized that every psychological adjustment involves a physiological change. From this point of view it is not inconceivable that the physiological changes of psychological adjustment could disrupt the physiological equilibrium and produce physiological disease. In this connection the extensive literature on the etiology of peptic ulcer and the studies of Robinson (35) and Donnison (10) are worthy of mention.

No discussion of etiology can ignore the troublesome distinction between functional and organic factors nor the attempts to classify disease into corresponding groups. Delafield (8, p. 8) refers to disturbed function *or* altered structure, but at another place (p. 6) he speaks of the functional abnormalities *and* the structural alterations which constitute the signs of disease (which constitute *disease* would be more accurate (29)). Gould (16) defines 'functional disease' as a derangement of the normal action of the organ without

structural alteration. These statements are confusing and ambiguous.

Clarification is possible. In the first place, it is absurd to conceive of a function without an organ or other structure that is functioning (5, p. 21). We may note functional alteration without being aware of the structural alterations involved, and it is possible that structural alterations may be discovered post mortem without specific functional alterations having been identified. Furthermore, that a given organ's structures are unmodified even though the organ shows a dyscrasia does not imply that there has been no structural alteration in some other organ (20). Such alteration need not be cellular; it may be chemical, within the cells, and thus not apparent microscopically. The classification of disease as either organic or functional is definitely misleading.

On the basis of information available from the relatively few authorities who have specifically discussed the concept of disease, it may be said that the current conception of disease is that of a manner of functioning, some aspects of which are apparent objectively and subjectively as symptoms of pain, dis-ease, and lowered efficiency. They are recognized as unusual and called abnormal, and result from a disturbance beyond the organism's limits of tolerance, empirically described.

#### MENTAL DISEASE

Explicit discussion of the mental disease concept has not been discovered; current views are implied in diverse writing in the whole field of psychological anomalies. It is hoped that this canvass of current opinion is representative; it is doubtless incomplete.

The most ready definition of mental disease is that it is a disease of the mind or a derangement or disorder of the mind. Obviously this definition contributes little to understanding. Text-

books of abnormal psychology and psychiatry quite uniformly describe certain syndromes under the heading of psychosis, and it is this form of behavior which is usually considered mental disease (16, 44).

Just as physiologically, disease is applied to conditions of pain and discomfort, so psychologically, a mental disease describes condition of pain and discomfort, though the latter are both physiological and psychological. There is loss of capacity for comfortable and efficient living (41, p. 865), loss of subjective well-being, alteration of evaluation of reality and contact with it (30, p. 17), and usually resistance to treatment (46, p. 129). Lack of insight, hallucinations and delusions, and marked change in demeanor toward others are additional common characteristics (13, pp. 149-150).

Mental disease is often referred to as a pathological condition (22, p. xi). Sometimes the implication is simply that behavior cannot be explained by any known motives, and the person is not responsible for his acts. The phrase 'pathological lying' is an example of this usage. The dignifying of ignorance reaches the nadir when such behavior is called *pseudologia pathologica*. Sometimes designating behavior as pathological means that there is tissue damage or alteration of structure. Foley (14) has criticized what he calls a pathological norm, saying that psychology has no interest in 'pathological conditions and their therapy.' This raises the question whether mental disease may exist without pathology, a possibility that is most unlikely. Suffice it to say that 'pathological' used in psychology often has different connotations from those current in physiology.

The term 'mental disease' is often applied to those conditions which are known as morbid (7, p. 119; 21, p.

308). The terms are essentially synonymous, as in physiology.

Physiologically, disease is a functioning of the organism. The same is true of mental disease. It is the attempt of the organism to adjust to new conditions (27, p. 29), a dysfunction of the integrated personality (47, p. xvii), or of whole conduct (38, pp. 186-7).

Mental disease, as dysfunction, is distinguished from normal and is identified with abnormal. Defining it as abnormal adds nothing to clarification of the concept for two reasons. The first is that a psychological norm of health is no more precisely defined than is the physiological. A statistical norm or central tendency, while serving as a crude beginning or reference point, is inadequate for a number of reasons. The main reason is that what is usual is not necessarily adequate adjustment. So-called statistical norms of specific traits are objective descriptions of adjustable criteria and not the criteria themselves. Truly adequate adjustment is an ideal, the definition of which is approachable but not attained, for cultural relativity and ethical considerations are involved. To say that mental disease is abnormal means only that it is an unusual condition having symptoms such as have already been described. A second reason why calling mental disease abnormal does not define the concept is that not all abnormality is considered disease.

To say that an individual has a mental disease usually implies more than he is maladjusted, or has a behavior disorder (39, p. 387). Bentley and Cowdry (4, p. 95) describe various forms of maladjustment and then add, "all are out of accord with their surroundings and with our desires; but we do not therefore set them down as 'pathological' or 'diseased.'" In another place (4, p. 3) they say that "to be socially maladjusted . . . may not

at all signify disease but something to be better understood and treated in quite another context." To call a condition a mental disease thus implies some special significance.

One view of this special significance is that only entities are diseases. One conception of entity has implied that a disease is something in the body and foreign to it. This is an unquestionably absurd superstition to be dismissed forthwith. However, entity has other meanings.

Entity may refer to a nosological unity, a specific train of symptoms (9, p. 6; 11; 33, pp. 31, 32-3). Such unities exist more frequently in textbooks than elsewhere (28), and are stereotypes for convenience (34, pp. 23-4). Even if invariant conjunctions of symptoms did occur, this would be no reason why only such syndromes should be called 'disease,' and there is no other reason for so doing.

Entity also implies, on occasion, specific etiology. This conception is usually associated with that of entity as a nosological unity and is an inference from it. It has been stated that conditions which are as yet ill-defined should be regarded, not as pathological entities, but only as syndromes (43, p. 258). Why only conditions having a specific known etiology should be called disease is not clear. Such a practice is not a necessary consequent of the generally accepted definition of disease as a disturbance of equilibrium nor consistent with it.

Specific etiology is sometimes considered as a characteristic peculiar to somatic disorders (33, pp. 32-3). On this basis an inference is made that if a clear-cut nosological unity (an entity) is observed, then specific etiology exists. This in turn suggests somatic causation, whereupon it is thought proper to call the condition a disease. (Somatic causation refers to etiology which does not

involve learning.) If this is done, mental disease becomes a subclass of psychosis, unless it is assumed that all the psychoses are of somatic origin. The effort to establish such a view may result from the physician's desire to maintain complete authority in the field of disease. One wonders, however, if the desire to define psychiatry as having to do with normal states (18, p. 10) is not an admission that the argument is weak. Considering mental disease as a result of somatic factors is probably most plausibly consistent with the view that individuals suffering from it are 'really sick' and not responsible for their acts. This also may be a reason why somatic factors are considered responsible for disease, though it is by no means a conclusive argument.

Mental disease as an entity rests upon an idealized and unrealistic interpretation of symptoms as nosological unities. It leads to a restriction of the term 'mental disease' to conditions of specific and known etiology as compared with that which is complex, general, and unknown. This is scarcely a significant or useful distinction. Finally, holding that only somatic causation gives rise to disease forces either a distinction between mental disease and psychosis, or the assumption of somatic causation of all psychosis. In any event, mental disease becomes, not a reaction to disturbance of psychological equilibrium analogous to physiological disease, but a subclass of psychological maladjustment.

A second view of the significance of a distinction between mental disease and other maladjustment is that the former differs from the normal in kind while the latter differs in degree. Babcock considers the insane as a group apart, mentally different from the normal. It is her view that, although the onset of a psychosis may be gradual, "at some time there is a change from a quantitative to a qualitative aspect, so that the

resultant condition is as different from the normal mind as ice is from water" (3, p. 158). Sherman believes that there is a continuum between the normal and the psychoneurotic, but that the "psychoses represent a different type of condition, a disease entity which symptomalogically and etiologically is only indirectly related to normal or neurotic conditions" (39, p. 368). Malamud (27, pp. 18-9) distinguishes between psychopathology and pure psychology and claims that the former should not advance theories in the latter. Furthermore, psychopathology cannot depend upon psychological explanations. Klein, as previously noted, concludes that there is a "genuine possibility of successfully contending that the facts of mental health belong in one category and those of mental disease in another . . ." (26, p. 13).

The belief that the psychoses differ from the normal in kind, on the basis of symptoms and etiology, is supported by an analogy with organic conditions. Allport advances this argument and it is more recently accepted and endorsed by Klein (26, p. 14). Allport's statement is as follows:

"Is the normal personality simply an undistinguished edition of the mentally diseased? We do not hold this view in reference to *organic* conditions. There is no continuum of states from cancer to no-cancer. The patient either has a malignant growth or else he hasn't; there are no intermediate conditions. Similarly a diseased mind is in many respects functionally quite different from (and not merely an exaggeration of) the normal mind" (1, p. 76).

The view just cited is not unchallenged. Instead of disease being fundamentally different from the normal, it is argued that the difference is one of degree only. Howard and Patry (22, p. xi) state that there is nothing in the abnormal which is not latently, potenti-

ally, or actually in the normal. Jastrow states that "the normal harbors and presents the abnormal in miniature and under control" (23, p. 314). More recently Schilder (37, p. 95) and Maslow and Mittlemann (30, p. 34) consider the abnormal as a mere accentuation of what is in the background or is unnoticed in the normal.

This belief is supported in two ways. It is held that neurotic and psychotic symptoms may be shown to be exaggerations of behavior present in the everyday life of normal people, that, for example, delusions are but exaggerated false beliefs. A second argument is from the generality of causal principles. Goldstein (15, p. 10) has expressed this most clearly by saying, "it has become increasingly evident that pathological phenomena can be recognized as an indication of lawful variations of the normal life process."

Thus, according to one view, there are conditions, the psychoses, which, if they occur at all, are recognized as abnormal and thus different in kind from anything ever considered normal, even when the norm is a statistical one and involves some inadequate adjustment. Only these are true mental diseases. Others, without raising the question of the applicability of the term 'disease,' stress the similarities between the normal and the abnormal; abnormal, presumably including the mental diseases, is merely more pronounced maladjustment.

If two items of behavior differ in degree they are two instances which exemplify the same concept. They can be placed upon a continuum so that one represents more and the other less of the conceptual reality. Thus to say that one individual is more depressed than another means that what we judge to be depression is less altered by changing situations, and persists in the face of more varied and stronger stimula-

tion. The more depressed person is hindered in the performance of more acts than is the less depressed person.

Difference in kind may refer to difference in etiology. Accordingly normal behavior arises from certain causal factors, while mental disease is assumed to arise from an entirely different set of factors. Sometimes it is assumed that these factors which produce mental disease are the aforementioned somatic ones. Differences in kind may refer to qualitative differences in symptoms. Mental disease, if it differs in kind, would therefore have attributes which are totally lacking in normal or in other maladjusted behavior.

These divergent beliefs about the relation of mental disease to normal behavior raise two questions. First, do the psychoses differ from the normal range of maladjustment qualitatively? Second, do they differ in etiology from those maladjustments which fall within the normal range?

It is most probable that the psychoses differ qualitatively from behavior within the normal range. This may be so even when a particular aspect of behavior may be shown to exist in the normal and to a greater or lesser degree in the abnormal. In physiological conditions, blood pressure deviations are present in a wide variety of syndromes that, because of the presence of other characteristics, differ from one another qualitatively. Psychologically, intelligence may differ in degree, yet within the compass of this variation there may be several qualitative patterns. When only one aspect of behavior is considered it may be possible to describe it on a continuum without making commitments about the syndromes in which it is present. They may be qualitatively quite different from each other. Paranoia, which is most readily viewed as differing from the normal in degree only, has characteristics which differ from 'normal' sus-

picion, namely complacency in the face of reason. Were it not for the fact that qualitative differences in syndromes are accompanied by quantitative ones, the controversy regarding which is fundamental would not arise.

Not only is it most probable that the psychoses differ from the normal qualitatively in terms of symptoms, but it is also most probable that these reaction patterns have an etiology different from that of any found within the normal range. In the first place, if certain abnormal conditions differ qualitatively from normal conditions it must be assumed that they have different etiology. To assume that qualitatively different patterns arise from the same causal factors would be tantamount to admitting that cause and effect are unrelated, a self-inconsistent proposition (36).

It is possible that qualitative differences may arise from quantitative differences in the same factors. Qualitative differences in symptoms might arise from differences in the amount of a single causal factor. Babcock's statement, previously quoted, suggests this possibility when she refers to the psychotic state as being reached by slow degrees but eventually attaining a point where qualitative differences appear. This suggests an analogy with critical points encountered in physical chemistry. There may be degrees which produce reactions which themselves prevent further conditioning or modification.

Even if it can be shown that the qualitative differences in symptoms arise from quantitative patterns of causal factors, it is not necessarily true that all qualitative differences are so produced. It is most likely that most if not all psychoses result from causal factors which are never present when the resulting reaction is called normal. This is certainly true if normal is taken to mean the ideal, adequate adjustment,

Perhaps the main reason why there is opposition to the view that the psychoses differ in kind from the normal is the belief that it implies special causal principles. Such an inference is not justified. Discrete syndromes, whether normal or abnormal, do not imply discrete causal principles. It is true that abnormal behavior, particularly the psychoses, may differ from the normal as ice does from water, but no more so. We do not have separate physical principles to explain ice. Ice, water, and steam all must be explained within the frame-work of molecular theory. Just so it is that psychological phenomena must be explained by causal principles which are general. It is unfortunate that such phrases as 'abnormal psychology' and 'adolescent psychology' have gained currency. Abnormal psychology does not have special principles. With respect to adolescence, its syndromes are explained in terms of learning principles operating in a set of circumstances, culturally and physiologically peculiar to a given age. Klein's statement that the facts of mental health and mental disease belong in different categories may apply to syndromes and to causal factors but not to causal principles, unless it is assumed that the psychoses are explainable in physiological terms only, an opinion which springs from a medical bias. Belief in general causal principles is no reason for denying that there are syndromes, specifically the psychoses, which differ from the normal in kind, both as to symptoms and etiology.

Sherman, in a previous citation, has stated that the psychoses are to be distinguished from the neuroses. In another place (39, p. 359) he states that the current psychiatric theory is that the neuroses differ etiologically from the psychoses, but recognizes that this view is not universally accepted. A number of writers (6, pp. 212-213; 12;

31; 42) belittle the distinction, claiming that it is legal and social, that it is quantitative and unclear, that it is unjustified by the facts, and has, Topsy-like, just grown up. Appel (2), however, reviews the current opinion on this subject and favors a continuance of the qualitative distinction.

If Appel's position is correct, then it is probable that restricting the term 'disease' to the psychoses must depend on whether the neuroses differ from the normal in kind, or whether it is assumed that there is some additional reason. Certain it is that generally speaking, only the psychoses are called 'mental diseases.'

Definition of physiological disease was accomplished in part by investigation of facts and opinions about its origin. To a large extent this has already been done relative to mental disease. The strong predisposition to favor somatic causation for the psychoses which has been noted is not universal. Harrington (17, p. 295) emphasizes the causative effectiveness of 'wrong education,' and the stimulus afforded by difficult and trying situations. Slight speaks of dysfunctions of the "individual living in a social environment, whose influences may be equally pathological as toxins, and other physical agents" (40, p. 34). The possibility of inadequate learning or conflict resolution as at least partially responsible is widely recognized. The confused issues involved in the concepts functional, psychogenic, and psychosomatic have been alluded to; further discussion is not germane.

Mental disease has been shown to have different connotations. Most generally it is applied to certain syndromes which are characterized by seriousness, lack of contact with reality, resistance to treatment, and certain special symptoms such as delusions and hallucinations. These syndromes are known as psychoses and are psychological proc-

esses. They are referred to as abnormal, though this adds nothing to an understanding of them save that they are unusual. Though not all abnormalities are psychoses, they alone are called 'mental diseases' because they differ from the normal in kind, or because they are entities. When, as it sometimes does, entity implies somatic etiology, it is possible that not even all the psychoses may be mental diseases. At any rate, 'mental disease' is restricted to a special group of maladjustments or abnormalities.

Such a concept of mental disease is not analogous to the physiological one. Physiologically a disease is the response of the organism to a disruption of equilibrium; psychologically it should mean the same thing. It should refer to an inefficient reaction to disturbance, not to a limited class only. The original animus for such a delimitation, the urge to define psychiatry in a physiological framework, seems to be past, for psychiatry admits no such limitation.

The fact is that when an individual's adjustive equilibrium is disrupted, at the physiological level by various agents, or at the psychological level by thwarting and conflict, there are four clusters of reaction syndromes, namely intelligent adjustment, simple maladjustment (the defense mechanisms or dynamisms), the psychoneuroses, and the psychoses. The latter two are usually recognized as abnormal, but whether or not they are is of social significance only. No one of the three kinds of non-adjustive reactions is more justifiably called a mental disease than any other. In fact there is no need for the term 'mental disease.' Disease is not an important concept in physiology; syndromes of dysfunction is a fully adequate expression. "Syndromes of dysfunction" is adequate for psychology, too.

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## "I" AND TIME: A STUDY IN MEMORY

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The following essay has two objectives: first, to contribute to a better understanding of memory by studying two of its most conspicuous features, namely, I and time; secondly, with this study as a basis, to help establish a concept of the mind that presents the mind as a unique phenomenon.

In order to study as complex a subject as memory, it is good to select as simple as possible an illustration. I am going to take an example of recall: I have met an old acquaintance. We reminisce. He says: "Do you remember our old friend Bill Jones?" I answer: "I remember him." For practical purposes language is very brief here. For my theoretical purpose of examining the nature of memory, language is too brief. In order to describe this act of recall adequately and completely, I have to say: "I remember myself having seen Bill Jones." If I want to have a genuine act of recall, it is not enough that I have an act of memory now, and that there was a Bill Jones in the past, and that this Bill Jones is the object of my present act of memory. But in a genuine act of recall I myself must have been in the past as well as Bill Jones and in some contact with him, and it is this past contact between me and Bill which I have to describe in an act of memory and which is the object of this act. There is a second reason for the fact that our everyday language is terse in describing the situation. Besides being concerned chiefly with practical purposes, we know that, being so terse, we want to point out only the most important piece of the object of an act of memory. I want to recall Bill Jones, and not myself, and reminisce with my

acquaintance about many of the qualities of Bill, because Bill is gone, while I, the other actor in this act of memory, am still here and do not require cumbersome conjuring. And yet for theoretical purposes, for the study of the nature of memory, the role which I am playing is much more important than Bill Jones'. It is the function of the I to make the act of memory tick. In any act of memory we have the I twice. I am the one who performs the act of memory. And I am the one who is part of the object of this act. The act of memory can be performed only because the subject of the act is identical with part of the object. The identity of the I is the link between the subject and the object. The importance of this statement becomes more obvious when we pay a little closer attention to the object. By now it could appear that Bill Jones and I make up the object in a way which could be described thus: I remember Bill Jones and myself exactly as in an act of perception I would say, "I see Bill Jones and myself," and there would not be much basic difference between an act of memory and an act of perception. The truth, however, is that the roles which Bill Jones and I play as parts of the object of memory are completely different from the roles we would play in an act of perception. In an act of perception we have the same function, which is expressed by the word 'and.' In an act of memory our roles differ, as far as our functions in being part of the object of the act are concerned. I am the subject of a new act and he is its object. That means the object of an act of memory, while it is the object of an act, consists itself of

another act. In other words, the act of memory as a whole consists of two acts: first of the act of remembering, secondly, of the act that is remembered. I remember an object, *and* I have seen Bill Jones. I is the subject of two acts, but in the second act *it* is not only the subject of the second act, but also part of the object of the first act. While the two acts have the same I as their subjects, they differ in another characteristic respect. The one act takes place in the present, the other act takes place in the past. We will see a little later that it is very crude to say that the second act takes place in the past, but for our present purposes it will suffice. Now we are able to understand why the identity of the subject in both acts is so important. Only by virtue of this identity in an act of memory can we link the present act to a past act in as intimate a way as it is done in an act of memory. This aspect of its structure is the most characteristic feature of an act of memory. We find the same fact in no other mental act, except in the act of anticipation, when the second act takes place in the future instead of the past.

Does my discussion so far imply that in an act of memory I am confronted with two acts taking place at the same time? Obviously not, because I stressed the point that they take place the one in the past, and the other in the present. But if that is true, am I permitted to say that I really have two acts in one? William James already stressed the point that any act of memory is a present act. There is no doubt about the reality of the first act but what about the reality of the second act? May I say that *having seen* Bill Jones is something real? No, because I do not see him now. The only real time is present time. To make this clearer by a more drastic illustration: I remember myself having driven a car against a tree

fifteen years ago. How fortunate I am that in an act of memory relating this experience the second act is not real! But if in any act of memory the second act is not real, are we forced to give up the notion, just acquired, that in an act of memory we deal with two acts? What is the nature of the second act? This leads us to a discussion of the peculiar stage of time which we call past. What do I mean when I say: I *have seen* Bill Jones, and that *having seen* him is the object of my present act of remembering? The object of the first act is something real only as the object of this act. But what is real is not the past. (Already the logical consideration that the past, if it was real, would have to be identical with the present, points against the possibility that the past is real.) Only an image of the past is real. This image is present, and it points beyond itself to the past, or this image says: what you see here as a picture of your mind once was present, or this is a symbol for an event that was once present and real. We are able to experience the past in a symbolic way because past is passed present. It is exactly the same as our present moment with the one all-important qualification that it is not real. It could be real only if it was the present moment. There is no past as a reality, there is a past only as a hypothesis. A hypothesis, to be sure, of high probability. The belief that things happened the way we visualized them in an act of memory is so strong in most cases that we do not consider the possibility of a mistake for a moment, and therefore we are shocked when in some cases we find out later that we were mistaken and things did not happen the way we 'remembered' them.

If only the present moment is real, how much is real? Only this one present moment. We are not allowed to say that a chain of present moments is real

with the present 'present' moment being the last link. For all the links of the chain with the exception of the last are nothing but images of a present. Even as the description of a highly possible hypothesis, the picture of the chain would be a poor picture. A chain is something spatial: all links are present together. In the experience of present time the 'links' would appear after each other. It is better to say one moment appears and disappears, then the next moment appears and disappears, and so with all moments. What is present at any moment is just this one moment, and time is a sequence of present moments, but only hypothetically speaking.

When we look now at the one precious moment left which is real without the help of a hypothesis, what insures us of its reality? Are we not in each act of memory faced with the symbol of something that is not real anymore, provided it was real at all? Is it not true that if we would turn to the subject of the act of memory, *i.e.*, to the I that remembers, we would make this subject the object of a new act of memory, and we would be beset by the same difficulties which we would find with any other object of an act of memory? If that was the case we would never know with absolute certainty that we really exist. It could happen as in Calderon's play that we would awaken one morning and would find out that what we considered our real life was nothing but a long dream. Fortunately for our sense of security in this world, we do not need assume such an extraordinary possibility. There is one aspect to any act of memory which gives us assurance as to the unhypothetical reality of the present moment, and that is the experience of the identity of the two I's present in all acts of memory. That is to say, what we experience is the fact that the I who is object and whose reality is only a highly probable one is identical with the I that

is subject and present now. While all acts of memory deal with past, they can do this only because they are permeated by a consciousness of the present. I must be aware of my present, else I could not have the experience of the identity of the past I with the present I. So there is at least this slender cord on which my consciousness of the reality of my life is hanging.

The mirror through which we see the past does not only tell us in a general way about the past; it gives us one specific characteristic. It says in the single case not only that something did happen in the past, but that it happened at a certain moment of the past. It happened before or after other events we remember; it was present and became past before or after them. If we could use the picture of a chain, we would have to say that all links have not only a definite position in regard to the link which represents the present but also a definite position among themselves. This order which we find in what we call the past is an impressive characteristic, but it should never induce us to base on it the reality of the past. For we can readily understand that a highly hypothetical past may show this order as well as a past real beyond doubt, and furthermore we know from experience that the belief in the order of events in the past is also subject to error even if this belief is very strong.

We are able to clarify the concept of the past (and the concept of time) still more, when we turn to another phenomenon that is often considered the opposite phase to the past, namely, the future. What is the future? We find the future in acts of anticipation: I am looking forward to my vacation. Man's interests, his highest hopes, his deepest fears are tied up with the future. And yet, what I call the future, is nothing but an image, and an image the reality value of which is even smaller than in

the case of the past. "There will be no tomorrow." As soon as the future becomes real, it is present. Only in a highly figurative sense are we allowed to say that the future 'becomes' real. In truth, our question would be what was the future before it 'became' present? Obviously the answer is: It was nothing. For if it had been anything real, it would have been present, and so it could not 'become' present. While in experiencing past, we deal with an image that points to a piece of time, the reality of which is at least probable, there is not even possibility for the reality of the future. That does not mean, of course, that the act of anticipation is not real. For the act of anticipation is a present act. Only its object is pure imagination, real as an imagination, but unreal as to what it points to. When we compare future with past, the quality of highly probable reality which we had to give to the past and which is so inferior to the reality quality of the present, at once takes on a different aspect. Being so far superior to the reality quality of the future, it appears a much more positive quality. The understanding of the future leads even farther into a clarification of the general concept of time than of the concept of the past. It does away with the very common picture of time in which time appears as a stream flowing from the past through the narrow gate of the present into the future. The complete inadequacy of such a picture is revealed the moment we realize that in order to be true, past and future would have to be present. For in any stream beginning, middle and end are present at the same time—in the present. But what constitutes the identity of past and future is just the fact that they are not present. To sum up our discussion on the nature of time: All that is real with high probability is the sequence of passed presents which end up with the present mo-

ment and which are tied for their very existence to this present moment. There is very little good material available for the study of the nature of time. The little we learn makes the rest appear even more mysterious. St. Augustine said about time: "If you do not ask me, I know; if you ask me, I cannot tell." I would accept the second part of the statement and change the first part by saying: "If you do not ask me, I have the delusion I know."

Memory is not only a treasure-house that gives us the tools for understanding I and time (as much as we can understand them); its importance goes much farther. It furnishes us with the foundations on which to establish the entire structure of the mind. In turning to this second part of my inquiry, I find that the connection with the first part (and so the justification for dealing with both parts in the same context) becomes apparent when we ask where we may find those foundations. We shall find them by a further discussion of I and time, with a greater emphasis now upon the I.

What is the I? Is it one of those basic phenomena of mental life of which we can say very little because we cannot trace them back to any more basic phenomena? While it is true that the I is a basic phenomenon of mental life, fortunately there is another method of describing phenomena besides tracing them back to more basic ones which we can apply here, and that is comparing them with other phenomena and clarifying our concepts of their nature by pointing out identical and contrasting features. When Fichte, who was the first thinker to see the peculiar nature of the I, tried to describe it by comparison, he presented the non-I, as the opposite to the I, and yet closely connected with it, as it could exist only at the mercy of the I. Today we have a much more striking concept for demon-

strating the I, but it shares with Fichte's non-I the strange combination of being very different from and very close to the I. It is the You. When I say You, I mean the extreme opposite to the I, but at the same time I mean another I. The fact that the I belongs to one mind and only to one mind is a characteristic of the I best understood when we realize the insurmountable wall which separates it from the You. The absolute lonesomeness of the single mind which results from the existence of this wall colors the whole of our mental life. There are no experiences of ours—no matter whether they are experiences with main emphasis on the intellectual or volitional or emotional aspect—which are not tinged by the consciousness of this lonesomeness. This lonesomeness is the spring of greatest despair when we try in vain to scale the wall—as in the case of the mother who wants the stubborn child to see the strength of her good intentions, or as in the case of the husband who wants his wife to see how he feels in the depth of his soul—it is at once the mainspring of greatest triumph when I am the one who wins the prize and not the other fellow, when I am the one who overcomes temptation or does good. When in our meditation on the absolute lonesomeness of the I, we happen to think of the You, we are startled by the significance of its meaning. For this You that I cannot reach is nothing but another I. The separation between I and You is two-fold. It is an absolute separation as to nature and as to existence. As to nature, they are two totally different phenomena: that I am I just means that I am not You. As to existence, there is no meeting of them in the world of experience. Against this two-fold background of distinction their similarity takes on its strongest significance. What the I is and what the You is can be understood only if we

know that I am another you, that you are another I. While we are unable to understand *how* such an antinomy can exist, at the same time each little piece of mental experience tells us *that* it exists. The inexplicable nature of the I, it seems to me, is one of the main reasons why many psychologists deny the existence of a mental realm different from the physical realm. And yet inexplicability cannot keep us from accepting reality if this reality is beyond doubt.

The I is the most perfect basis for establishing a mental realm in its own right because it is absolutely different from what we find in the physical realm, and it pervades all aspects of mental life. There is no mental act which is not the act of an I. This leads us to a new feature of the I which will deepen our understanding of its peculiar nature and which will strengthen our conviction of the basic differences between mental and physical world. While the I is completely separated from anything outside of its mental realm, it is in the most intimate relation to other phenomena of its own realm. The I is always subject of a mental act: I think, I remember, I will, I see, I hear, etc. There is never any experience of the I in which the I appears all by itself. We could not grasp what it would be like if the I would exist without the mental act of which it is the subject. In turn, we could not grasp what a mental act would be like without an I as the subject. What would we mean by saying, "It thinks." What could this 'it' be if not another I? When we realize how the one cannot exist without the other, we are not surprised to see other evidences of the intimate nature of the relationship between the two. It is impossible to say where the one ends and the other one starts. I am *in* the act, we are one, we are not two parts lying side by side. The fact that a certain act and I are

one, however, does not prevent I from becoming the subject of another of my mental acts, and here again I engages into the most intimate relationship. While the I is the subject of all my mental acts, it cannot play any other role in the mental realm. It will always be only the subject of mental acts. To make this clear, no mental act is better suited than memory. At first glance it looks as if we have two I's here, one as the subject of the act, the other as part of the object. Yet as we have seen, the part of the object is not the I, but only an image of the I, an image which tells us: "Once upon a time when I was an I, I was the *subject* of another mental act." This image is the farthest the I comes in being something different from the subject of a mental act. It is not very far, because all the image does is to point to the only function of the I, namely, being subject of a mental act. The role which the I plays as a subject of a mental act is another characteristic of the mental realm which is not found in the physical world.

But we have not exhausted the significance of the I for the establishment of an independent mental realm as yet. What does it mean that the I is the subject of *all* mental acts? It means that the I forms the tie which binds all mental acts together. It means that the I is the tool which makes for *one* mental realm, consisting of all mental acts. Here the I counteracts the effects of time. Time makes for the separation of the mental acts. At this present moment I am thinking. This is the only mental act which I am performing this moment. It is separated by time from the mental act which I may be performing in the next present moment, and so on. While time separates the mental acts, the I as subject of one mental act always points to the others. When I am thinking at this moment, it means that the I which is thinking now is the

same I which was recognizing, recalling, anticipating, willing in previous acts. Because of the I experience each mental act, while being a separate mental act, reaches out beyond its own borders. So the I constitutes the mental realm as a whole. At the same time, being an I and not a You, it constitutes the mental realm as a peculiar whole, as a whole set off from other wholes and unable to merge with them. The manner in which the parts of the mental realm are tied together is another evidence of the different nature of the mental and the physical realms. There is nothing in the physical realm that is comparable. What comes closest to it is its spatial nature. As space is something characteristic for all parts of the physical realm, so is the I characteristic of all parts of the mental realm; space, however, separates the parts of the physical realm, while the I ties together the parts of the mental realm. In space one thing lies beside the other and separated from it; if the two should come closer together, space would disappear.

The two phenomena which are characteristic of the mind are time and space in their respective relationships. We have seen the paramount significance of time for memory. We need point out only one more feature of time in memory (and in all other mental acts) to make clear its unique nature in relation to the mind as a whole: Time of the mind is subjective time; that means not only that it could not exist without the subject experiencing it, but also that the very nature of time is determined by the experiencing subject. The length of time—and length is the most important feature of time—varies according to the experiences of the subject. I do not need to elaborate on this fact. We all know how time can drag or fly depending on our interests, anticipations, fears and joys in regard

to the content of our various experiences. The significance of the fact in our context becomes clearest when we compare subjective with objective time, the time of the physical world, the time which is measured by our clocks and watches. There is no greater difference imaginable than the one between those two phenomena. While subjective time rests on its variability in length, objective time presents the opposite as its most important feature. Each hour, each minute, each second, is as long as any other hour, minute or second. This feature makes objective time a tremendous help in our lives. Where would we be in our practical lives if we had to rely on subjective time! In spite of its great practical value, however, this objective time is never experienced. When I look at the second dial of my watch for five minutes, none of the five subjective 'minutes' which I experience is identical with the five objective minutes because, depending on my mental attitudes, those five 'minutes' differ in length, and therefore lack the most indispensable feature of what we call objective time. By its uniqueness subjective time helps establish the unique nature of the mind. Subjective time proves its close relation to the mind by its complete dependence on it.

The role which space plays in the realm of the mind is even more peculiar than the role of time. Mind is not in space. It has been shown many times how foolish it would be to attribute spatial qualities to mental acts. It is completely reasonable to say that an act of thinking, or volition, or emotion, takes long or short time; it is as unreasonable to say that my thought measures two inches or my emotion weighs two pounds. The non-spatial

nature of mental acts is one of the greatest stumbling blocks for many who try to recognize a realm of the mind different from the physical realm. It is so large an obstacle because all through our lives we have faced the world of space (for the satisfaction of most of our motives), and so we fall prey to what William James calls the psychological fallacy, namely, to 'see' space everywhere and to imagine it as present in a realm in which it is not present. I am convinced that spacelessness constitutes as important a characteristic of mental acts as their temporal nature does; yet my statement needs qualification at one point. The mental act is void of space, but its object very often is in space. In the illustration of memory with which I started, my meeting with Bill Jones is the object of the act of recall, and this object is in space. This is the more remarkable because the act of recall and the object of the act form one inseparable unit. There is no act without an object. The act collapses, so to speak, when the object is taken away from it. On the other hand, this close relationship does not induce me to consider the act spatial because its object is spatial. For there is, in spite of the most intimate relationship, a clear distinction between them. Why would it otherwise make sense to talk of each of them separately? And besides, the impossibility of attributing spatial features to mental acts, mentioned above, stresses the difference. So with due consideration of this qualification, the fact remains that the difference between the mental and physical worlds, in regard to the function of space, forms, in part, the basis on which to found the concept of the uniqueness of the realm of the mind.

## THE CONCEPT OF PLAY

BY HAROLD SCHLOSBERG

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In a recent paper (1) Dr. Beach published a critical examination of past and present concepts of play. In it he pointed out that "recent American textbooks on comparative or physiological psychology" have neglected the topic, and attributed the neglect to "the exclusively observational character of available data and the obvious inadequacy of existing interpretations." Beach then presented the better-known theories of play, and urged the necessity of testing these theories by controlled observation.

In the present writer's opinion, Beach does not go far enough in his criticisms. The program Beach suggested could scarcely be carried out, and it would be of little value even if it were practicable. The basic difficulty is found in the definition of the concept 'play.' Beach does not define the term, but states that "Any serviceable definition of play must be based upon a number of predominating characteristics which combine to set it off from non-playful behavior." Nevertheless, he believes that play can eventually be defined in objective terms, as can other complex activities (among others, he mentions maternal behavior, courtship and mating, and social dominance).

Unfortunately, play is a concept of a type different from that of maternal behavior or courting. One can define maternal behavior fairly well in terms of an integrated pattern of stimuli and responses of a particular type. But behavior is generally called playful only if it seems useless in the eyes of an observer. Thus, maternal behavior, mating, and social dominance are primary categories that describe fairly specific

behavior sequences, while 'play' is simply a descriptive term that may be applied to behavior in any one of the primary categories, as long as the behavior seems incomplete or otherwise useless. It is obvious that behavior may be incomplete or inadequate for a number of reasons. These reasons may be phrased in the conventional terminology of stimulus-response psychology, without any reference to such a vague concept as 'play.'

1. *Generalization.* If an organism already responds to one stimulus in a certain fashion, it will respond to similar stimuli in a similar way. The mechanism and limits of generalization have not been worked out in detail, but it seems to be a fairly basic characteristic of our receptor-neuro-muscular system. A puppy chasing a rolling ball is an excellent example of generalized response to small moving objects. It would seem gratuitous to ask whether or not he is practicing skills necessary for future hunting! Of course such activity *may* develop necessary skills, and it may be worth-while to investigate the question, but it would seem likely that we would find more about the activity of chasing a ball by studying the conditions which determine the act itself.

2. *Thresholds.* The strength of a stimulus necessary to evoke a response varies with the condition of the organism. One factor which influences the threshold is fatigue. This factor would seem to be closely related to the 'excess energy' theory of play. A rested animal will respond to many stimuli which are too weak or unimportant to elicit behavior in a tired animal. A rested condition is neither essential nor peculiar to

play, as Beach pointed out, but it is probable that a rested condition makes the animal more responsive to stimuli which are not closely related to a strong drive, or to partial stimuli. To this extent the 'excess energy' theory of play is valid, as long as we do not put the cart before the horse and say that the animal plays to use up excess energy.

Thresholds also vary as a result of external or internal changes of a general sort. For example, the male Siamese Fighting Fish (*Betta splendens*) will desert the bubble nest containing eggs if the temperature drops a few degrees. Or he may carry out other partial activities, such as building the nest but not mating with the female, if the diet has been inadequate. Presumably hormonal and neural factors are of great importance, but they have not been studied adequately in this species. Do we understand these partial patterns better if we say, "The male is only playing at breeding"?

Beach himself has contributed much to our knowledge of the neural and hormonal factors which control sex behavior in the rat. For example, he finds that immature males will show complete mating behavior considerably before the normal age of puberty, if they are treated with testosterone propionate. In discussing his results he says (2, p. 290):

"It appears that the neuromotor mechanisms mediating copulatory behavior are well organized and ready for functioning long before puberty; and they do function at a relatively low level prior to the occurrence of postpubertal testicular activity. That the resultant behavior is incomplete and infrequent may be due to the presence of high thresholds in the circuits in question. The function of androgen is thus interpreted as a lowering of these thresholds. The hormone does not organize the behavior, but does facilitate its appearance by reducing thresholds in the essential neuromotor circuits."

It would seem that this sort of research is more fruitful than wondering why animals exhibit sex play, or play at hunting, or fighting, or what you will.

3. *Learning*. If we forsake our interest in the 'practice for later life' explanation of play, and instead examine specific cases of partial or generalized behavior, we find many interesting problems that are related to learning. For example, extensive generalization is characteristic of the early stages of many conditioned responses. It is only after considerable training, often by the method of differential reinforcement, that conditioned responses become precise and differentiated. It may well be true that early initial generalization is at least a partial explanation of the frequently reported 'playfulness' of young animals. The relative importance of maturation and of practice in cutting down this early generalization is in great need of investigation.

Learning comes into the picture in another way. Often apparently useless activity is reinforced by obscure means, so that it is continued for a long time. For example, a dog may bring a ball to his master, who will throw it, and then pat the dog for retrieving it. Whether the pat on the head acts as reinforcement in its own right, or through functional autonomy, is not the question here; certainly the pat serves as a reward, and reinforces the sequence of behavior that was set off either by the sight of the ball or of the dog's master. Many of our adult forms of play (as bridge) are reinforced by even more obscure methods, but they are undoubtedly reinforced in some fashion if they continue. The current emphasis on children's play as a diagnostic and therapeutic tool shows most clearly that this apparently aimless behavior is often highly motivated. Incidentally, it furnishes an excellent example of the advantages of studying specific behavior

in a direct fashion, instead of arm-chair theorizing. The technique could scarcely have evolved on the basis of general theories of play, or from our knowledge of play in young animals.

One could go on in this fashion for many more pages, showing how the facts subsumed under the term 'play' can be handled more effectively in specific stimulus-response terms. But enough

has been said to indicate that the category 'playful activity' is so loose that it is almost useless for modern psychology.

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with the original set had not been. In fact, several points of interest were found. First, the mean annual precipitation was found to be significantly higher than the original set. This is due to the fact that the new set has more precipitation in the winter months, while the summer months have less. Second, the mean annual temperature was found to be significantly lower than the original set. This is due to the fact that the new set has more cold days and less warm days. Third, the mean annual wind speed was found to be significantly higher than the original set. This is due to the fact that the new set has more days with strong winds.



